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THE

SCIENTIFIC PROCEEDINGS

OF THE

ROYAL DUBLIN SOCIETY

VOL. 26 (N.S.)

No. 1. 1st May, 1952

FITZGERALD MEMORIAL NUMBER

(Plates 1, 2, and 3)

A.—FITZGERALD AS REVEALED BY HIS LETTERS TO HEAVISIDE. F. E. HACKETT

B.—THE FITZGERALD CONTRACTION. HERBERT E. IVES

C.—THE FITZGERALD-LORENTZ CONTRACTION—SOME PARADOXES AND THEIR RESOLUTION. W. H. MCCREA

D.—THE FITZGERALD - LORENTZ CONTRACTION PHENOMENON AND THEORIES OF THE RELATIVITY OF GALILEAN FRAMES G. J. WHITROW

E.—EFFECTS OF ACCELERATION IN THE MICHELSON-MORLEY EXPERIMENT, J. L. SYNGE

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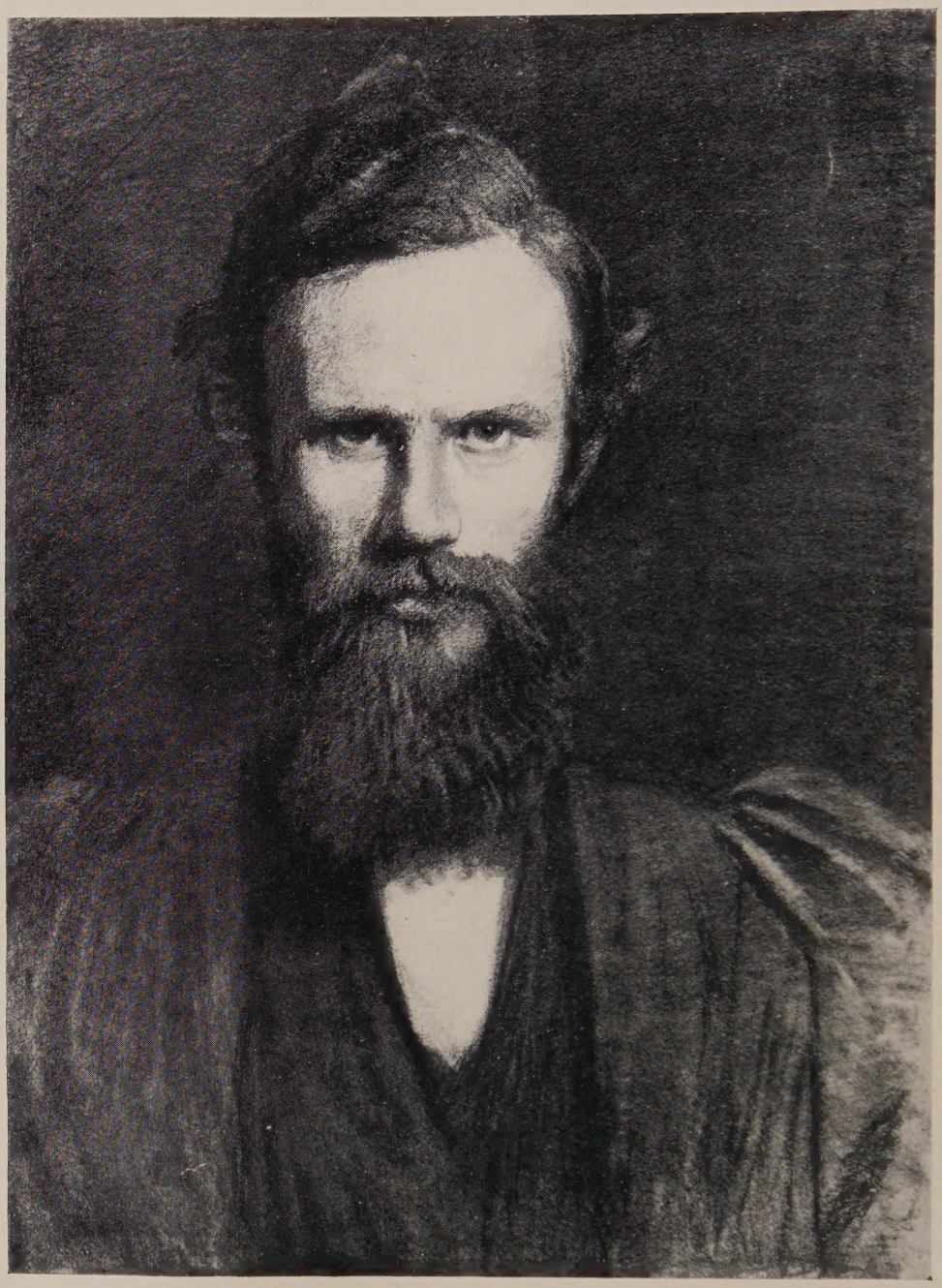

George Francis FitzGerald
(From a Charcoal Drawing by J. B. Yeats, 1878)

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(PLATES 1, 2, AND 3)

A special meeting of the Society was held on Tuesday, 30th October, 1951, the Chair being occupied by Major Arthur McMorrough Kavanagh, M.C., President of the Society, and afterwards by Dr. A. E. J. Went, Chairman of the Committee of Science and Its Industrial Applications.

The President, in opening the proceedings, reminded all present that the meeting was called to commemorate the centenary of the birth of George Francis FitzGerald, who had been a well-known pioneer in physical science, a distinguished Irishman, and a most valued member of the Society.

The following message of greeting was read from the President of the Institution of Electrical Engineers, of whose Irish branch Professor FitzGerald had been the first Chairman :—

> " I learned with great pleasure of the intention of the Royal Dublin Society to commemorate the Centenary of the birth of Prof. George Francis FitzGerald and of their consideration in inviting the members of the Irish Branch of the Institution of Electrical Engineers to be present and to collaborate in this act of commemoration.
>
> FitzGerald's name will long be remembered, not only for his original and deep-seated contribution to the explanations of natural phenomena which lie at the base of the science of physics, but also for the help and encouragement he gave to so many developing physicists, and indeed electrical engineers, through his constructive criticisms of their work and help with their problems. I have recently glanced again at the address he gave when on the 22nd February, 1900, he delivered his Inaugural Address as first Chairman of the Dublin Local Section of the Institution, later to become the present Irish Branch. From this it is clear that, viewed in the climate of thought prevailing

at that time, his original and advanced conceptions were not confined to the realm of physics, but extended to the whole field of the well-being of his fellow men.

(Signed),

J. HACKING,

President."

The President of the Physical Society was represented by Prof. K. G. Emeleus of the Queen's University of Belfast.

Prof. F. E. Hackett conveyed to the meeting personal messages of congratulation which he had received from Prof. E. T. Whitaker and from Dr. G. F. C. Searle, who remembered FitzGerald personally.

The first speaker was Dr. E. H. Alton, Provost of Trinity College, Dublin, who remembered FitzGerald personally, and said that he was proud to be the head of the College at which FitzGerald had done his great work. He remembered how optimistic FitzGerald had been on the possibility of human flight, which he considered was largely a matter of balance. He was also greatly interested in colour photography, and particularly in Joly's pioneer colour process, but it was chiefly his character as a man with wide interests outside his laboratory that appealed to all.

Slides were shown illustrating FitzGerald's experiments with a glider constructed by Otto Lilienthal, the famous German pioneer.

Among other exhibits shown during the meeting were a model constructed by FitzGerald in 1885 illustrating his views on the properties of the ether, and reproductions of some of FitzGerald's letters to Heaviside, kindly presented by the Institution of Electrical Engineers, and of a letter to Sir Joseph Thomson, copied through the courtesy of his daughter, Mrs. Charnock.

Photostat copies were also shown of pages of the Dublin *Evening Mail* and Dublin *Daily Express* reporting news of a regatta in Dublin Bay obtained by wireless, during the summer of 1898, FitzGerald having co-operated with Marconi in this the first demonstration of the value of wireless for the transmission of news.

The greater part of the meeting was devoted to the reading of the following papers which are included in this special centenary number :—

A. Prof. F. E. Hackett—FitzGerald as revealed by his letters to Heaviside.

B. Dr. H. E. Ives—The FitzGerald Contraction.

C. Prof. W. H. McCrea—The FitzGerald Lorentz Contraction—Some paradoxes and their resolution.

D. Dr. G. H. Whitrow—The FitzGerald Lorentz Contraction phenomenon and theories of the relativity of Galilean frames.

E. Prof. J. L. Synge—Effects of acceleration in the Michelson Morley experiment.

No. 1.A

FITZGERALD AS REVEALED BY HIS LETTERS TO HEAVISIDE.

By

F. E. HACKETT.

The letters from FitzGerald to Heaviside are preserved amongst the Heaviside papers and manuscripts in the possession of the library of the Institution of Electrical Engineers. The correspondence, which began in 1888 and continued for the next twelve years, is described by Heaviside in a letter given in the introduction to the collected scientific writings of FitzGerald, edited by Larmor.

" I only saw him twice knowingly, once for two hours and again for six hours, after a long interval, yet we had a great deal of correspondence at one time, and I seemed to have quite an affection for him. A mutual understanding had something to do with it. You know that, in the pre-Hertzian days he had done a good deal of work, not large in bulk but very choice and original, in relation to the possibilities of Maxwell's theory, then considerably under-developed and little understood, and his way of looking at things was more like my own than anybody's . . . . . But I knew all the time that he had a wider field than myself, and no time to specialise much."

The second meeting took place immediately after the meeting of the British Association at Bristol, as shown by a telegram from FitzGerald, announcing his visit preserved amongst the correspondence, dated the 18th September, 1898. The care with which Heaviside, this strange man of genius, deaf, a recluse at Torquay from scientific society, preserved the letters from FitzGerald as well as from other correspondents, is evidence of the value he attached to these links with the scientific world.

The rarity of the meetings between these two made their letters more expansive, so that we find those from FitzGerald range from deep discussions of Maxwell's theory to autobiographical confessions in a conversational style. We can almost fancy this was the way FitzGerald talked, fascinating and impressing his contemporaries in Dublin as we are told by Praeger, or at meetings of the British Association as described by Lodge.

In *A Populous Solitude* Praeger tells of the conclave which gathered around FitzGerald at a simple luncheon table during the academic session—" where discussion on almost every subject within the confines of the universe, and sometimes outside of them, raged without ceasing " . . . . . " The genial FitzGerald, essentially a product of Dublin and Dublin University, was a big bearded man, and his resonant voice filled the little room as he poured forth wisdom and nonsense in a delightful medley, for he was a brilliant talker " . . . Around FitzGerald as Chairman were grouped W. J. Sollas, A. C. Haddon, Grenville Cole, Thomas Preston, Frederick Trouton, as well as many occasional visitors including Ridgeway and Larmor if they were passing through Dublin.

The meetings of the British Association gave FitzGerald periodical opportunities of exerting his powers amongst the physicists of the day. " In the old days ", writes Lodge in *Advancing Science* (1931), " there were real discussions especially when Sir William Thomson or FitzGerald was present. They nearly always had something of value to say or something to start, or they spotted a point of interest which escaped the others present and which drew further remarks from the members present and from the author of the paper."

The Dublin meeting (1878) was memorable to Lodge because " there it was that I made the acquaintance of my great friend G. F. FitzGerald " to whose memory he dedicated *Advancing Science*—his personal reminiscences of meetings of the British Association. It was at the Bath meeting (1888) that FitzGerald, as President of Section A, brought forward and made known to the world the brilliant experimental discoveries of Hertz confirming Maxwell's theory, even then not clearly understood, as shown by the picture given by Lodge, that at this meeting "Lord Kelvin who had hitherto been hostile to Maxwell's theory began to be shaken in his opposition to it and went about with the second volume (of Maxwell's Treatise) under his arm, every now and then appealing to FitzGerald to explain a passage."

To these reminiscences then must be added the detailed account of the birth of the hypothesis of the FitzGerald contraction given by Lodge in his Presidential address to the Birmingham meeting (1913). " The FitzGerald-Lorentz hypothesis I have an affection for. I was present at its birth, indeed I assisted at its birth, for it was in my study at 21 Waverley Road, Liverpool, with FitzGerald in an arm chair, and while I was enlarging on the difficulty of reconciling the then new Michelson experiment with the theory of astronomical aberration and with other known facts, that he made his brilliant surmise : ' Perhaps the stone slab was affected by the motion '—I rejoined that it was a 45° shear that was needed. To which he replied : ' Well, that's all right—a simple distortion.' And very soon he said : ' And I believe it occurs and that the Michelson experiment demonstrates it.' "

The FitzGerald-Heaviside correspondence is naturally largely concerned with the technicalities of Maxwell's theory. Appleyard has given extracts from it in his memoir of Heaviside in *Pioneers of Electrical Communications*. Here and there appear the sparkle and humour of the talk cherished by Dubliners. (September 26th, 1892)—" I hope you will succeed in making the ordinary mathematical physicist think in vectors though I do not think your notation an improvement. You see, I was ' riz ' on Tait and get very much muddled by your omission of S: and when one gets bothered every turn one naturally takes a dislike to the botheration . . . . . " (March 15th, 1895)—" You say Maxwell's (magnetic theory) is all a muddle. So it is, but there is an underlying stratum of explicability, I think, which must have been unconsciously guiding him." (June 8th, 1896)—" I had a long correspondence with Lord Kelvin and his last letter says that he gives up everything he ever wrote about the ether. I hope he is not still quite so down in the mouth about it." (May 7th, 1898)—" Lord Kelvin with his usual impetuosity has rushed at a single fluid theory of electricity—but he is very rash."

FitzGerald had, however, a fellow-feeling for impetuosity, as will be seen from

a self-revealing passage from one of his early letters discussing his mode of working. The letters between FitzGerald and Heaviside soon begin to show that interplay of ideas which gives brilliance to the cut and thrust of good talk. A rapid change of view came naturally to FitzGerald, who was so quick in scientific discussions. Writing to Heaviside he falls into the Heaviside manner of enlivening a deep disquisition on the ether by asides or flashing remarks which we can see now, if developed, would have led into the key-positions of the theory of relativity. The extracts which follow illustrate these activities of the mind of FitzGerald, as he relaxes in correspondence.

(January 26th, 1889)—" I was waiting to look up some of my lucubrations ; many of them are out of date, and most of them more or less idiotic, notably the first two on originating wave disturbances. Most of the idiocy in them is however corrected in the third, and they are more or less interesting as geological fossils." (February 4th, 1889)—" I admire from a distance those who contain themselves till they have worked to the bottom of their results, but as I am not in the least sensitive to having made mistakes I rush out with all sorts of crude notions in the hopes that they may set others thinking and lead to some advance. I am entirely and purposely ignorant who did anything first. I only know that I very often bother over a thing for ever so long to find that I might have got the answer and been bothering at deductions if I had only been lucky enough to see it."

This candid confession of his mode of work confirms what is said of him in the *Dictionary of National Biography*, " He possessed extraordinary versatility, and in the deepest subject was more at home than in the trivial, throwing out luminous suggestions with splendid prodigality, and rejoicing if they were absorbed and utilised by others."

(February 4th, 1889)—" You ask ' what if the velocity is greater than that of light ' I have often asked myself that but got no satisfactory answer. The most obvious thing to ask in reply is ' Is it possible.' If all motions are due to the ether it may not be able to originate motions at a greater rate than that of light, its action on the moving body might cease and with it an acceleration of the motion."

An anticipatory approach to the fundamental postulate of the special theory of relativity.

(September 26th, 1892)—" I entirely agree with your infinitesimal vectors in differentiation. I can't for the life of me see what is the use of all that fluxion hugger-mugger." (August 25th, 1893)—" I think it is a pity that someone with a store of patience and energy for infinite calculation does not work on the average motion of liquids. There is evidently a field quite comparable to that of the average motion of particles which is the kinetic theory of gases, and I feel horribly confident that it is *the* theory of the ether. I have a sort of feeling in my bones that it must be so. I suppose I am a bit of a crank about it."

Here is the intuitionist in FitzGerald breaking out. His anticipations of the importance of this field is confirmed by the modern theory of liquids.

(December 20th, 1893)—" As regards mass the whole of the present system is in such a hopeless state with engineers' masses and scientific masses, and those who think there is some esoteric occult meaning in quantity of matter, that a good flow

of fresh air is required to get rid of the cobwebs, and a new term like inertance would, I think, be a great assistance in teaching students."

(December 22nd, 1893)—" Have you ever considered what would be the extra mass of an atom owing to its atomic charge ? A charge of electricity or magnetism acts like an added mass (really due to the ether motion of the charge of course, *i.e* the atom has increased inertance being bound with moving ether). Now this should interfere with Kepler's laws unless (1) the added ether gravitates (which is unlikely, though considering that the atom itself is only a motion in the ether (?) there would be nothing odd in the added motion also gravitating), or (2) The added motion is proportional to the original mass of the atom. This latter is in some degree relative to Gladstone's law of refractive indices, but the law is not as accurately true as that, *maybe* a departure from Kepler's laws may exist, and it might be worth while repeating Newton's experiments on pendulums to determine the specific gravitance (? gravitivity) of different kinds of matter."

These extracts refer to a letter which appeared in *Nature*, December 12th, 1893 (conjointly with Dr. F. T. Trouton) on Systematic Nomenclature. It proposed that the electromagnetic nomenclature put forward by Heaviside

conduction	conductance	conductivity

should be extended to general physics giving for example :

inertia	inertance	inertivity
gravitation	gravitance	gravitivity

Here FitzGerald is making that cardinal distinction between what Einstein has termed " inertial mass " and " gravitational mass " which led Einstein on the road to the General Theory of Relativity.

This is an instance of a brilliant idea not further pursued. The developed nomenclature of Heaviside has greatly clarified the terminology of electricity and magnetism. Perhaps this forgotten proposal of FitzGerald may some day be revived. New terms like inertance, inertor, gravitance, gravitor, paralleling the electrical terms resistance, resistor, conductance, conductor, would help to bridge the gap between elementary physics and relativity.

Neither in the *Collected Papers* nor in the correspondence with Heaviside does any allusion to the Michelson and Morley experiment occur before 1900. In that year FitzGerald wrote reviews of *Aether and Matter* for *Nature* and *The Electrician*. In each he refers in a detached way to Larmor's discussion of the contraction hypothesis. Possibly the writing of these reviews stimulated him to propose in the autumn of 1900, as Trouton tells, " an electrical experiment, with the object of detecting any relative velocity there might be between the Earth and ether." " On the last opportunity," writes Trouton, " I had of discussing the matter with Professor FitzGerald, preliminary experiments had been made, giving as far as they went negative results ; the final results not being completed till after Science had to deplore the grievous loss it sustained at his death. FitzGerald, on that occasion, made a remark which, as well as I remember, was to the effect that should the negative results then obtained be sustained by further work, he would attribute the non-occurrence of any

observable effect to the same general cause as produced the negative results in Michelson and Morley's experiments on the relative motion of the Earth and the ether by means of the interference of light."

The only reference to the Michelson and Morley experiment in the Heaviside correspondence occurs in a detached sheet undated but evidently belonging to the year 1900, since it contains an allusion to Cremieu's experiments.

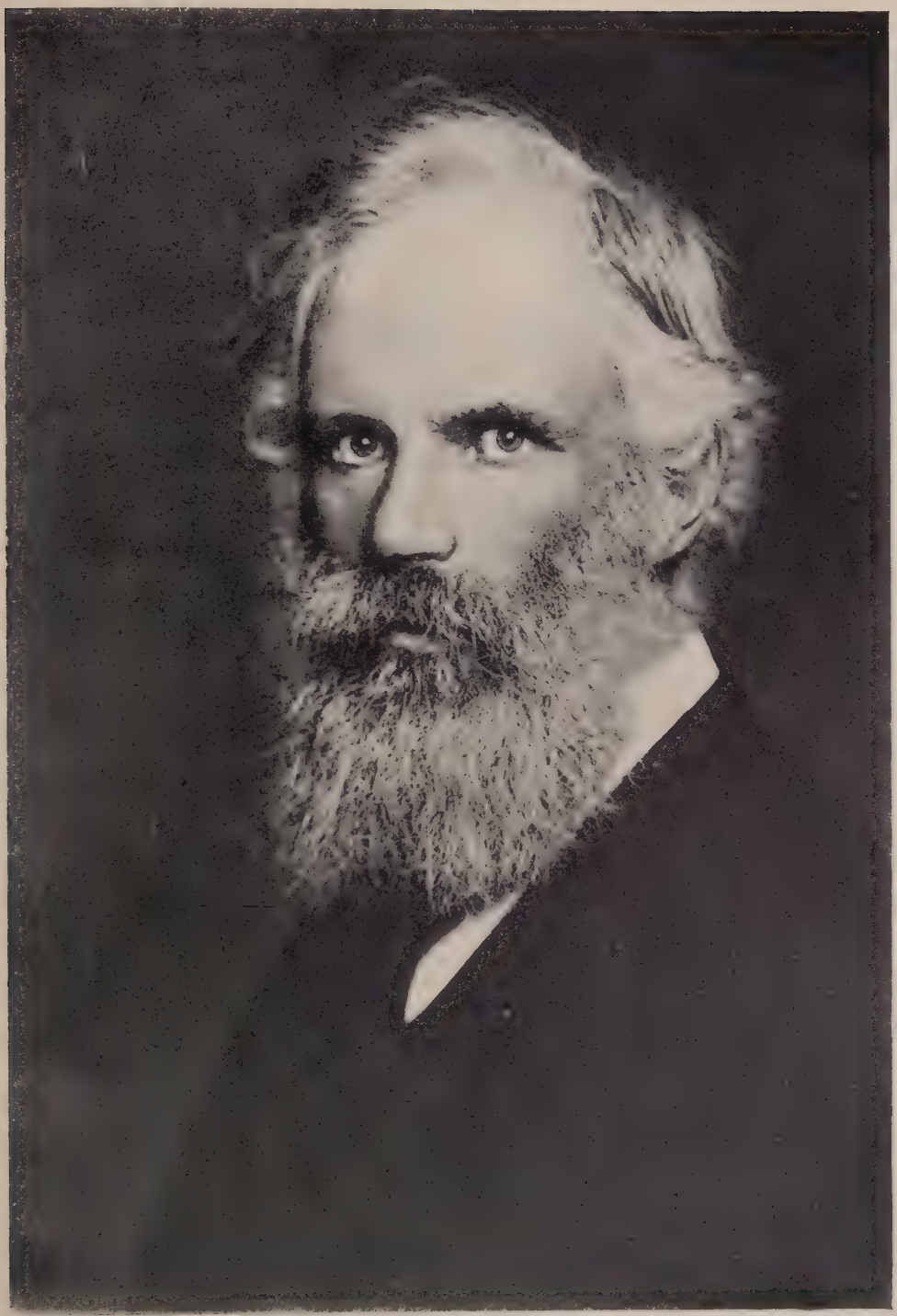
". . . I am fairly well satisfied with Michelson's and Morley's experiment. No doubt changes of temperature are going on, but they kept a large circular stone floating in mercury in continuous slow rotation and made observations in various azimuths and saw *no* change in the interference bands. It is very unlikely that there could have been just that irregular distribution of warming and cooling the stone and what it supported that, as it rotated round, the expansions and contractions should have exactly compensated for the effect they expected to see owing to the drift of the ether past the Earth. It would have required a most curious adjustment of the radiation, etc., to the time of day or night at which they made their observations, which seems extremely improbable. It is, however, a not impossible coincidence. Another possibility might be some irregular gravitation to objects in the room that distorted the stone but this is still more improbable. *All* experiments should be repeated under different circumstances by different observers, but Cremieu and Rowland's experiments are much more important to have repeated just now than this one.

P.S.—Put down any more than ordinary idiotcy to my stomach which is troubling me again."

Thus to the last FitzGerald, writing in the physical idiom of the time, kept his friendship with Heaviside in repair. It is evidence of his great and generous nature that he linked in friendship so many of his scientific contemporaries of such diverse temperaments as Heaviside, Kelvin, Ramsay, Larmor and Lodge. We owe to Lodge many vivid descriptions of FitzGerald as a scientific personality, including the profile of FitzGerald as President of Section A at the memorable meeting of the British Association at Bath—a man exhibiting an altogether exceptional combination of qualities, professional knowledge, great originality, mathematical and experimental skill both of the first order, quietness of comprehension, and a ready wit.

Possessing these great intellectual endowments, FitzGerald had as well a distinguished presence. According to a College colleague: "His appearance was not unworthy of his fame. More striking he was than handsome, but his ample grey locks and beard, his furrowed brow, his penetrating eyes reminded one of the bust of some Greek philosopher which we cannot look upon without that instinctive feeling of respect which intellect and character command among civilised men."

Another comparison, however, springs to mind. Looking at the photographic portrait it recalls in a remarkable way by its impressiveness and penetrating eyes the Turin self-portrait of that incomparable and universal genius of the middle ages—artist, scientist, and engineer—Leonardo da Vinci.



George Francis FitzGerald
(From a photograph taken in 1897)

1.B

THE FITZGERALD CONTRACTION.

By

HERBERT E. IVES

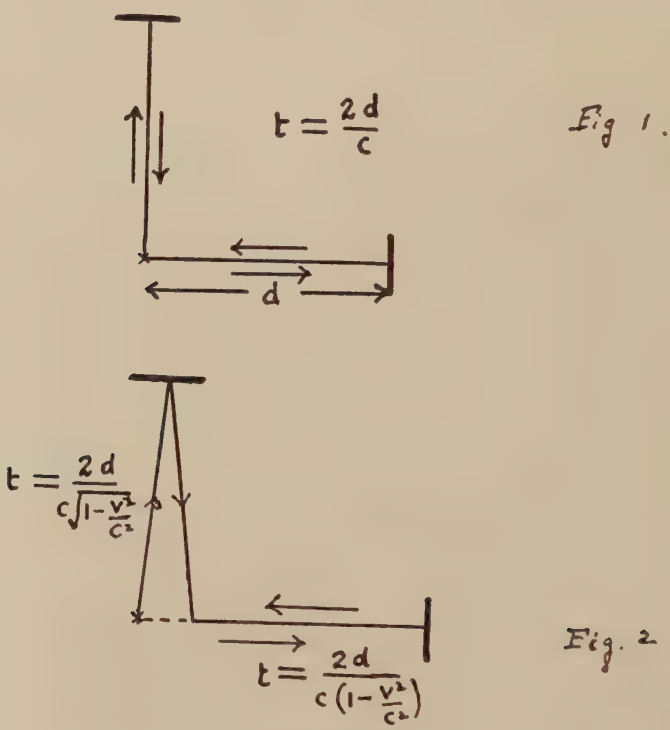
I.—Historical Survey.

In 1895, in a paper on "Michelson's Interference Experiment"(1), H. A. Lorentz stated the problem raised by the nul result of that experiment in these words: "Now does this result entitle us to assume that the ether takes part in the motion of the earth, and therefore that the theory of aberration given by Stokes is the correct one? The difficulties which this theory encounters in explaining aberration seem too great for me to share this opinion, and I would rather try to remove the contradiction between Fresnel's theory and Michelson's result. An hypothesis which I brought forward some time ago, and which as I subsequently learned, has also occurred to FitzGerald, enables us to do this." In a footnote he says "As FitzGerald kindly tells me, he has for a long time dealt with this hypothesis in his lectures." Rather curiously FitzGerald never himself published his hypothesis; it was made public, as FitzGerald's idea, by Sir Oliver Lodge, and by Lorentz, in the passage above quoted. Nevertheless his claim is not contested to have first introduced the hypothesis that matter is contracted in its direction of motion—the FitzGerald or FitzGerald-Lorentz contraction.

This hypothesis, which FitzGerald threw off in so casual a manner, has had in the hands of others, extraordinarily wide ramifications in physics, in mathematics, in philosophy, in the realm of the mystical. It is my purpose to review some of these developments, historically and critically.

The FitzGerald contraction was devised to explain just one thing, namely the nul result of the Michelson-Morley experiment. If, in Fig. 1 a pulse of light is sent out to a mirror and back, and at the same time a pulse goes out at right angles to the first to another mirror and back, the positions of the two mirrors can be so

arranged that the two returning pulses arrive simultaneously at the origin. If now the whole system is set in motion, with the velocity v, along the direction of one of the light beams, the condition shown in Fig. 2 obtains; the pulse in the direction of motion takes a time $\frac{2d}{c\left(1-\frac{v^2}{c^2}\right)}$ to go out and back, the pulse going to the other mirror takes a time $\frac{2d}{c\sqrt{1-\frac{v^2}{c^2}}}$, where c is the velocity of light; the two returned pulses would not coincide, as the Michelson-Morley experiment shows they do. Now the simplest compensatory behavior, which would make them coincide, is the one proposed by FitzGerald, namely that the apparatus shall contract, in the direction of motion, by the factor $\sqrt{1-\frac{v^2}{c^2}}$. This however is not the only change of dimensions that would produce this result; the arm of the apparatus



FIGURES 1 AND 2

in the direction of motion might remain unchanged, the other might expand, or both arms might change so that the volume of the apparatus remained constant,

the one essential condition being that the ratio of dimensions along and at right angles to the direction of motion must be $\sqrt{1-\frac{v^2}{c^2}}$.

Now the consequences of adopting one or other of these possible changes of dimensions, while they would not show in the Michelson-Morley experiment, would show in another experiment, if it were performed with sufficient accuracy. This is the measurement of the velocity of light by the customary laboratory method of sending a light pulse out to a mirror and measuring the time of the out-and-back transit by means of a clock. If we assume that, as in the Michelson-Morley experiment, we would get the same result whether the apparatus is stationary or in motion, FitzGerald's contraction requires that the clock must also suffer a change while in motion; it would have to run slower, in the ratio $\sqrt{1-\frac{v^2}{c^2}}:1$. If the contractions of dimensions, while still in the ratio $\sqrt{1-\frac{v^2}{c^2}}:1$, were of different amounts from FitzGerald's, different alterations of clock rate would be called for.

The fact that clocks, as well as rods, could be affected by motion does not appear to have been considered by FitzGerald, but it was recognized by one of his contemporaries, Sir Joseph Larmor, who in his essay "Aether and Matter" of 1900 (2), accepted the FitzGerald contraction factor, and as a consequence the variation of clock rate by the factor $\sqrt{1-\frac{v^2}{c^2}}$. Larmor in his discussion of this was almost as casual as FitzGerald had been. He merely mentions that "the scale of time" is altered in this way, "which will involve the Doppler effect." He made no great point of it, and it appears to have been practically unnoticed by his readers of that time.

Before studying these contractions in detail I wish to emphasize the really fundamental step which FitzGerald made. His immediate object was to elucidate the Michelson-Morley experiment. But he did much more; he introduced the idea that material objects, which include the instruments by which we make our measurements, are not ideal fixed things. The corollary, which was long in being drawn, is that in formulating the relations of physics in equations, these equations should recognize the behaviours of the measuring instruments, for it is only by the indications of instruments that we verify our theoretical predictions. A further consequence is that we must scrutinize carefully the terminology of physics, which was formulated on the assumption of invariant instruments and measurements. As an illustration, where formerly we could speak blithely of "velocity" as the quotient of distance over time, we must, if we suspect our rods and clocks to be subject to alteration when in use, employ a more cautious term, such as "rod-clock-quotient," and if more than one rod or clock is necessary, that fact must appear in the equations. These consequences of FitzGerald's hypothesis contain the germs of what are now called "the operational principle" and "semantics."

FitzGerald did not live to contribute further ideas on the general subject of measurement with variable rods and clocks. After him the chief names in this field are those of H. A. Lorentz and Henri Poincaré. Their approach to the subject was from a somewhat different angle.

Lorentz applied himself to the problem of finding formulae for the phenomena of electromagnetism which should be the same whether the frame of reference was stationary or moving. He was in fact endeavouring to extend the invariance with motion found in the Michelson-Morley experiment to all electrodynamic phenomena. He did not give any name to the generalization or principle which he was endeavouring to support, and his procedure was to make a series of specific hypotheses or inventions. The first of these was what he called " local time." Starting from the observation that the distance d traversed by a moving particle or signal on a stationary body was the distance d-Vt on a parallel moving body whose velocity was V, he postulated as " a new independent variable " the " local time " on the moving body as $t—Vd/c^2$, and was thereby enabled to set up electromagnetic equations which were of the same form on both bodies, as far as terms in V/c were concerned. He then made his second invention, the postulation of the contraction in length which now goes by his name in conjunction with FitzGerald. He next applied this same contraction factor to his " local time," and so evolved a set of " transformations," which, used in electromagnetic equations, yielded the same results on both stationary and moving bodies. His " local time " had no physical explanation, it was simply something he had to have to give the result he wanted. Poincaré pointed out that it could be obtained if one, ignoring the fact that the velocity of light must actually be different with respect to relatively moving bodies, called it the same for all, and fixed the " local time " by a light signal—an observation which neither Poincaré nor anyone else took seriously. The introduction of the contraction factor in the time variable was not scrutinized as to significance, and indeed Lorentz's transformations as he published them were rather crude in form, lacking symmetry and mathematical elegance.

Henri Poincaré approached the subject from a more philosophical standpoint. As early as 1895 he expressed the opinion that the relations of physics must be independent of the frame of reference. He followed Lorentz's work closely, criticising and clarifying it. With regard to Lorentz's transformations he expressed the opinion that what Lorentz arrived at by successive inventions should be capable of attainment by applying some all-embracing principle. In 1904 he crystallized his opinion by formulating " The Principle of Relativity," in the following words :

" The principle of relativity, according to which the laws of physical phenomena should be the same, whether to an observer fixed, or for an observer carried along in a uniform motion of translation, so that we have not, and could not have, any means of discovering whether or not we are carried along in such a motion."(3)

Fortified with this principle Poincaré then took Lorentz's transformations and gave them a thorough mathematical going-over, which resulted in symmetrical expressions forming a mathematical group. These revised transformations, which

he published (4) early in 1905, Poincaré suggested be called "the Lorentz transformations," even though as he said they were "a little different" from those of Lorentz. They have been so called ever since (5).

Of Poincaré's work Lorentz said "For certain of the physical magnitudes which enter in the formulae I have not indicated the transformation which suits best. This has been done by Poincaré, and later by Einstein and Minkowski. My formulae were encumbered by certain terms which should have been made to disappear . . . . I have not established the principle of relativity as rigorously and universally true. Poincaré, on the other hand, has obtained a perfect invariance of the electro-magnetic equations, and he has formulated "the postulate of relativity," terms which he was the first to employ."(6)

Let us now look at these Lorentz-Poincaré transformations: they are

$$x_2 = \frac{x_1 - Vt_1}{\sqrt{1 - \frac{V^2}{c^2}}}$$

$$y_2 = y_1$$

$$z_2 = z_1$$

$$t_2 = \frac{t_1 - \frac{Vx_1}{c^2}}{\sqrt{1 - \frac{V^2}{c^2}}}$$

where x_1, y_1, z_1, are distances traversed (in one direction) by a particle or signal in one frame of reference; x_2, y_2 z_2, distances in the second frame of reference; V the velocity of the frames with respect to each other; t_1, and t_2, are similarly the times (i.e. the differences in times between the passages of the particle across the two ends of its path) in the two reference systems, and c is the velocity of light.

These equations have played a prominent role in theoretical physics; according to Larmor they actually constitute the principle of relativity. They state among other things, that rods are contracted in the direction of motion by the factor postulated by FitzGerald, that clocks run slow by the same factor. It must however be remembered that the FitzGerald contraction factor was put in them by Lorentz, that they embody Lorentz's invention of a "new independent variable," "local time," for which no satisfactory explanation has been offered heretofore, and that the contraction factor for clock rate takes its value for reasons of mathematical symmetry. A stream cannot rise higher than its source.

I propose to leave these transformations for the present, to return to them later, with this observation, which I shall justify in the sequel, that the Lorentz-Poincaré transformations are incomplete expressions, mathematical limiting cases of complete expressions which, conforming to Poincaré's principle of relativity, can

be obtained without *ad hoc* inventions. We shall find that the complete transformations can be derived from the contractions which FitzGerald introduced. *It is these contractions, rather than the transformations, which are the fundamental implements of the principle of relativity.*

II.—Derivation of Rod and Clock Contractions, and Lorentz-type Transformations.

I now take up the derivation of the exact value of the FitzGerald contraction, investigating at the same time the accompanying change of clock frequency, perceived as a consequence by Larmor. I shall use a line of attack which I believe is essentially that which FitzGerald had in the back of his mind, but in place of the mere invariance of the Michelson-Morley experiment, I shall use Poincaré's extrapolation from this—his principle of relativity. This principle I shall use in conjunction with the accepted physical fact that the velocity of light is independent of the velocity of the source. The procedure will be to consider in order the measurement of the transit time of a light signal sent out-and-back on a measuring platform, and of a signal sent in one direction.

On any randomly chosen moving platform, consider a light signal originating at a source at one end, at which is a clock, sent to a reflector at the far end, and back to the clock. Because of the independence of the velocity of light from that of source or mirror, we may expect the forward velocity of the light signal to be, in general, different from that in the back direction. *The term "velocity" is used here, and in all our references to this initial platform, to mean the quotient of divisions on the platform by time as indicated by the single clock fixed at the origin.* Call the out and back velocities c_o and c_b, neither individually known. Then for a platform of read length d, the out and back times, in terms of the single clock at the origin, will be d/c_o and d/c_b. Calling the total time out and back t, we then have

$$t = t_o + t_b = \frac{d}{c_o} + \frac{d}{c_b} = d\left[\frac{c_o + c_b}{c_o c_b}\right]$$

We now call on the principle of relativity, which demands that this time shall be the same whatever the motion of the platform. Call this time

$$t = \frac{2d}{c}$$

then

$$\frac{c_o + c_b}{c_o c_b} = \frac{2}{c}$$

on all platforms.

Taking up our position on this initial platform, consider another platform of length l, moving parallel to it with a velocity W, in terms of divisions on the initial

platform and the clock at its origin. We then have for the out signal on this platform

$$l+Wt_o=c_ot_o$$

and for the back signal

$$l-Wt_b=c_bt_b$$

the total transit time is then

$$t=t_o+t_b=\frac{l\left(\frac{c_o+c_b}{c_oc_b}\right)}{\left(1-\frac{W}{c_o}\right)\left(1+\frac{W}{c_b}\right)}$$

$$=\frac{2l/c}{\left(1-\frac{W}{c_o}\right)\left(1+\frac{W}{c_b}\right)}$$

By the principle of relativity this out-and-back signal time, when measured by a clock on the origin on the second platform, must be the same whatever the value of W, that is, W must disappear. The conclusion *to which we are forced*, is that the measuring instruments on the moving platform must be altered by their movement. We accordingly assign to them new readings of length and time, according to the notation

$$l=l'f_D(W)$$

$$t=\frac{t'}{f_T(W)}$$

where the primes indicate read values, and where

$$f_D(W)f_T(W)=\left(1-\frac{W}{c_o}\right)\left(1+\frac{W}{c_b}\right)$$

so that

$$t'=\frac{2l'}{c}$$

from which W has disappeared.

The individual values of $f_D(W)$ and $f_T(W)$ are not obtainable from the consideration of out-and-back signals alone; we are still faced with the possibility that either one might be a constant, while the other carries the whole burden of the change. To determine their individual values we next consider a light signal proceeding out from one end of the platform to the other, with the velocity c_o-W relative to the platform. At what time will it reach the far end?

This simple question brings us face to face with the problem of establishing the relations in time, of events in different places, a problem—that of the meaning of "simultaneity"—which has long engaged the attention of philosophers. If we had at our disposal signals of infinite velocity the problem would present no difficulty. With signals of finite velocities, in general different by unknown amounts in the out and back directions, the problem of establishing the time of a distant

event, in terms of the indications of a single fixed clock, becomes an *indeterminate* one. The extent of our knowledge, as emphasized by A. A. Robb (8), is that a distant event (arrival of a signal) occurs *after* the signal is sent out, and *before* its return. We shall here recognize and accept this indeterminancy of distant times. We shall not attempt to fix these real although indeterminate times, either by any physical procedure or by any arbitrary " definition." Instead of " time " we shall concentrate our attention on our necessary measuring rods and clocks, and find what these, when used under our control and observation, will indicate. This is an application of the " operational principle " championed by P. W. Bridgman ; the outcome will be equations describing events exclusively in terms of observed and measured quantities.

Returning now to the one-way light signal : We have, in studying two-way light signals, been concerned with a single clock at one end of a platform. We must now have a clock at the far end, and it must be set in some definite relation to the first. By adhering to the operational principle we bar the setting of this clock by any " definition " or ritual, such as ascribing an arbitrary value to a light or other signal. We must set the clock by some operation susceptible to observation and measurement. There is just one procedure which meets this requirement. It is the performable and observable operation of moving a clock from one end of the platform to the other, at a rate *measured by the clock itself*, that is, by the number of scale divisions passed over by the clock, divided by the elapsed time as indicated by the clock itself. The self observed rate of movement of this clock, which we shall call the " setting clock," will be designated by the " rod-clock-quotient " " q ". This " q " of the setting clock is described not only by its self made observations, but is capable of description, when $f_D(W)$ and $f_T(W)$ are fixed, in terms of the velocity of the clock with reference to the initial reference platform ; it thus forms a bridge between the indeterminate co-ordinates of the initial platform, and observable quantities. Our final equations will all have terms in " q " and its role will be to eliminate paradox and mysticism from the problem of measurements on relatively moving bodies.

As a further aid to transferring our thought and attention to the instruments of measurement and their readings, it is desirable to use a new symbol to avoid, where ambiguous, the use of such terms as " time " and " velocity ", which, inherited from the pre-FitzGerald era, and used too naively and indiscriminatingly, have been the cause of confusion. The quantity to be symbolized is the difference in readings between the two clocks traversed by a signal at the beginning and end of its path. This two-clock measurement I shall designate by the Greek letter χ ; the two-clock rod-clock-quotient is then l'/χ. This new symbol will also appear in our final expressions, and will assist in dispelling confusion and paradox.

Coming back now to the problem of determining the individual values of $f_D(W)$ and $f_T(W)$ by one way light signals, we note that if a moved clock is altered in its rate the effect is cumulative. (unlike the behaviour of rods, which re-assume their dimensions of rest or steady motion after being moved into position end to

end), and the moved clock will indicate a different *epoch* from the unmoved one. We investigate, on our original randomly chosen reference platform, the amount of the change of epoch, due to the use of the setting clock, in terms of the as yet individually unknown functions $f_D(W)$ and $f_T(W)$, and determine the signal transit time as indicated by the difference in reading between the distant clock, so set, and the clock remaining at the origin. We then assume that the *independence of motion* found in the Michelson-Morley experiment for out-and-back signals shall also hold for one-way signals. We do not ascribe a definite value to the measurement to be expected, we prescribe only that the principle of relativity shall hold, that is that W shall disappear from this two-clock expression for transit time. From this investigation, for the details of which the original papers must be consulted (7, 8), we find the exact values of $f_D(W)$ and $f_T(W)$, which are

$$f_D(W)=f_T(W)=\sqrt{\left(1-\frac{W}{c_o}\right)\left(1+\frac{W}{c_b}\right)}$$

that is, the lengths of the measuring rods and the frequencies of the clocks on the moving platform, are altered by the *same* factor.

This factor, indicating either contraction or expansion, depending on the relative values of c_o and c_b, is somewhat puzzling to interpret as to physical meaning. It becomes physically significant if we take as our initial reference system not any randomly chosen platform, but the one for which $c_o=c_b$, that is one *stationary* in the light transmitting medium. When this is done, we have

$$f_D(W)=f_T(W)=\sqrt{1-\frac{W^2}{c^2}}$$

that is, the factor indicates a *contraction*, of exactly the value picked by FitzGerald.

In the use of these functions in deducing relations between observable and measurable quantities, to which we shall next proceed, it makes no difference which of the above values of $f(W)$ we use. Those just given amount simply to a change in the value of W, but since W must disappear in our final relations, because of our adherence to Poincaré's principle of relativity, we are at liberty to start from any platform we please. Mathematical convenience alone suggests the use of the platform with reference to which the alterations due to motion are simple contractions. But transcending mathematical convenience in importance is the deduction that there exists among all co-ordinate systems on which we could base our studies, one of unique physically significant character.

We now proceed to use these contraction factors to find expressions to represent the actual measurements we can make of signals or moving particles on two relatively moving platforms. We consider a particle or signal projected from one point to another ; in its trajectory it makes a " trace " on each of the two moving platforms. On each of the platforms we measure the length of the trace by rods laid end to end.

Also on each platform we set a clock at the end of the trace by moving a setting clock over the trace at a self measured rod-clock-quotient q. These traces and these clock settings we express in terms of distances, times, and velocities in the initial randomly chosen co-ordinate system. From these equations we derive finally a set of equations solely in terms of the observable and measurable quantities—the read distances, c, q, and χ. What we do, described in general terms, is to use the real but indeterminate co-ordinate system of our randomly chosen initial platform as a temporary structure, in terms of which the phenomena under observation, and the manipulations of the instruments used for measurement, are each described. We then eliminate—like erasing the pencil guide lines of a drawing—the indeterminate co-ordinate system, leaving—fully inked in—the relations between phenomena and the observable measurements of real instruments.

We arrive at a set of revised Lorentz-Poincaré transformations(9, 10) as follows :

$$x_2' = \frac{x_1' - \left\{ \dfrac{d_1'}{\chi_d + \frac{d_1'}{q}\left[\sqrt{1+\frac{q^2}{c^2}}-1\right]} \right\} \left\{ \chi_1 + \frac{r_1'}{q}\left[\sqrt{1+\frac{q^2}{c^2}}-1\right] \right\}}{\sqrt{1 - \left\{ \dfrac{d_1'}{\chi_d + \frac{d_1'}{q}\left[\sqrt{1+\frac{q^2}{c^2}}-1\right]} \right\}^2 \frac{}{c^2}}}$$

$$y_2' = y_1'$$

$$z_2' = z_1' \qquad \left(r_1' = \sqrt{x_1'^2 + y_1'^2 + z_1'^2},\ r_2' = \sqrt{x_2'^2 + y_2'^2 + x_2'^2} \right)$$

$$\left\{ \chi_2 + \frac{r_2'}{q}\left[\sqrt{1+\frac{q^2}{c^2}}-1\right] \right\} = \frac{\left\{ \chi_1 + \frac{r_1'}{q}\left[\sqrt{1+\frac{q^2}{c^2}}-1\right] \right\} - \left\{ \dfrac{d_1'}{\chi_d + \frac{d_1'}{q}\left[\sqrt{1+\frac{q^2}{c^2}}-1\right]} \right\} \frac{x_1'}{c^2}}{\sqrt{1 - \left\{ \dfrac{d_1'}{\chi_d + \frac{d_1'}{q}\left[\sqrt{1+\frac{q^2}{c^2}}-1\right]} \right\}^2 \frac{}{c^2}}}$$

In these expressions every symbol represents a performable operation whose result is observable on a measuring instrument. The primes denote measured quantities ; the distances by measuring rods laid end to end, the time intervals by

clocks at the positions of events. In order, we have on the left $\begin{matrix} x_2' \\ y_2' \\ z_2' \end{matrix}$, the cartesian co-ordinate measurements of the event in system B; $r_2' = \sqrt{x_2'^2+y_2'^2+z_2'^2}$ is the measured distance between the origin of co-ordinates and the event; χ is the difference in reading of the clocks at beginning and end of the particle trajectory; q is the self measured velocity of the clock moved to the point of the event; c is the velocity of light as measured by out-and-back signals along any path. On the right we have the similar quantities for system A, with the addition of d' and χ_d These are the read position and corresponding read clock time of the origin of platform B along the axis of motion on platform A, that is d'/χ_d is the read two-clock rod-clock-quotient of one platform with respect to the other.

Certain features of these transformations deserve particular attention. Notice that the "time" transformation contains in the numerator of the right-hand side (when we disregard the small terms involving q) the "local time," the "beautiful invention" as Poincaré described it, which Lorentz made in order to secure invariance of the electromagnetic equations, the physical basis of which was however obscure. Here it comes about not as an *ad hoc* invention, but through the use of the setting clock to establish the epoch of a distant clock. Notice that the "distance" and "time" transformations both contain in the denominator a "contraction" factor of similar form to the FitzGerald contraction factor, but expressed in terms of measured distances and clock readings, in place of the velocity W (with respect to the stationary co-ordinate system) in which the FitzGerald contraction itself is expressed.

These complete Lorentz-Poincaré type transformations are expressions of considerable apparent complexity. A preferable description is that they contain a great deal of explicit information. Complexity of this sort is the price we have to pay for equations which tell a complete and unambiguous story in symbols all of which have a definite operational meaning. We can however reduce these transformations to a simpler form, with interesting results, if we adopt new symbols for the long-bracketed terms. Thus if we write

$$\frac{d_1'}{\chi_d + \frac{d_1'}{q}\left[\sqrt{1+\frac{q^2}{c^2}} - 1\right]} = \overline{V}$$

$$\chi_1 + \frac{r_1'}{q}\left[\sqrt{1+\frac{q^2}{c^2}} - 1\right] = \overline{T}_1$$

$$\chi_2 + \frac{r_2'}{q}\left[\sqrt{1+\frac{q^2}{c^2}} - 1\right] = \overline{T}_2$$

the transformations become

$$x_2' = \frac{x_1' - \overline{V}\overline{T}}{\sqrt{1 - \frac{\overline{V}^2}{c^2}}}$$

$$y_2' = y_1'$$

$$z_2' = z_1'$$

$$\overline{T}_2 = \frac{\overline{T}_1 - \frac{\overline{V}x_1'}{c^2}}{\sqrt{1 - \frac{\overline{V}^2}{c^2}}}$$

These it is seen at once are of exactly the form of the Lorentz-Poincaré transformations. The consequences of the latter, which were listed earlier in this paper, with regard to the contractions of lengths and clock frequencies, are *mimicked* in these new equations in terms of the new variables which have just been operationally defined.

I have deferred to this point, where we are prepared to dispose of it, a literal consequence of the original Lorentz-Poincaré transformations, which has been responsible for a vast amount of confusion of thought and mysticism.

It is this: if the ratio r_1'/t_1' in one system is c, the out-and-back velocity of light, then the ratio r_2'/t_2' in the other system is also c; that is the velocity of light relative to any moving platform, however measured, is a constant. This is a conclusion completely at variance with our classical concept of velocity; it has been justly characterized by serious philosophers as a "paradox" and "ununderstandable." It is sufficient reason for the conclusion that the Lorentz-Poincaré transformations, as left by Poincaré, while conforming to the principle of relativity, which was Poincaré's chief concern, do not contain all the factors necessary for the proper representation of phenomena, that is, as I remarked earlier in this paper, they are incomplete.

Now the transformations which I have just given, which I have described as complete, do not pose this paradox. In these it is not the quotient of read distance by difference of clock readings, r'/χ—a quotient which at one time would have been unhesitatingly called "the velocity of light"—which is the same on all platforms, it is the measurement represented by the function

$$\frac{r'}{\chi + \frac{r'}{q}\left[\sqrt{1 + \frac{q^2}{c^2}} - 1\right]}$$

which, when made on light signals, yields the value c on all platforms. This function

comprises readings on rods and readings on several clocks. It is derived in full recognition of the fact that the velocity of light is different with respect to different bodies in relative motion. That this multi-term function should assume the constant value c on all platforms is understandable, while the paradoxical " constancy of the velocity of light " is not.

At this point let me make a few remarks on another treatment of the problem which would place a different interpretation on the contractions. I refer to what is called the " Special Theory of Relativity." This consists of a structure built on two " principles." The first of these is " the principle of relativity " which is, in wording and content, the " principle of relativity " enunciated earlier by Poincaré. The second is " the principle of the constancy of the velocity of light," which consists in the postulate that the velocity of light, however measured, always has the constant value c. This second so-called principle is, as was pointed out above, what the literal interpretation of the Lorentz-Poincaré transformations states. It is a rationally unacceptable interpretation from which we are freed by the formulation of the transformations in their complete form. Working backward from this " principle " the special theory of relativity obtains exactly the Lorentz-Poincaré transformations. As noted earlier they contain the FitzGerald contraction and the parallel change of clock rates. According to interpreters of the special theory of relativity these contractions are not " real " as they were to FitzGerald and Lorentz, but are artefacts, conjured up by the authority of the " principle."

The justifications claimed for the " principle of the constancy of the velocity of light " are two ; the first, that it is required by the principle of relativity. This is not so. Poincaré's principle does not require this ; it requires that an out-and-back measurement on one platform shall give the same result as an out-and-back measurement on any other similar uniformly moving platform ; and that a one-way measurement on one platform shall give the same result as a one-way measurement on any other platform ; it does not require that the two kinds of measurement, involving different numbers and manipulations of instruments, shall give the same result. The " sames " here are different things, only identified by the loose use of words.

The second justification is that we " always " obtain the same constant value " c " by experiment. Now the only experiment of adequate precision is the usual laboratory out-and-back measurement, while the problem dealt with in the special theory of relativity is that of one-way measurements, of which none of adequate precision have ever been made, so that the " principle " is not in fact supported by experiment. Furthermore, according to our analysis, if a one-way measurement were made, using rods and clocks experiencing the FitzGerald contraction (irrespective of how this may have been derived), it would not yield the value " c ", so that the principle instead of being supported, would be contradicted by experiment. I dismiss, as mere travelling in a circle, the attempt to save the principle by decreeing that clocks can only be moved " infinitely slowly," that is, must remain fixed in position, and that distant clocks are to be set in epoch by light signals ascribed the

velocity c, so that when light signals are measured they shall be found to have the velocity c.

From this brief review of the "Special Theory of Relativity" I draw the conclusion that it does not provide an acceptable alternative derivation or interpretation of the FitzGerald contraction.

III.—Experimental Verification.

I turn now to experimental evidence for the FitzGerald contraction. It is to be noted that the contraction was postulated to explain a nul result. As the quotation from Lorentz with which I started this paper shows, an alternative hypothesis would be that the light transmitting medium is carried along with the earth. Lorentz, who rejected this possibility, asserted that the Michelson-Morley experiment in fact proved that the FitzGerald contraction must be a reality. Nevertheless, either to overcome any lingering doubt as to some other possible explanation, or because of the dissatisfaction which must surround a purely nul experiment, it is desirable that some positive manifestation of the postulated contraction be found.

Now as to the contraction in length, no practical experiment has as yet been proposed; it would involve the movement of measuring rods at speeds a sensible fraction of the velocity of light, say a thousand kilometers a second, in order to produce a contraction of any manageable rod which would be visible in a microscope. This line of experiment is not promising. On the other hand the slowing down of a clock, which we have found to be according to the same factor as the FitzGerald contraction, and a necessary corollary of it (if either is proved the other follows) presents experimental possibilities.

The original suggestion of such a test is due to J. Stark (5), who pointed out that a rapidly moving positively charged hydrogen atom—a canal ray—is an atomic clock, whose frequency is exhibited in the light it emits. Stark's suggestion was that the frequency of the canal ray light be compared with that of similar stationary atoms of hydrogen, and thereby the frequency of the moving hydrogen clock be determined. Stark expected, from theories of his own, a much larger alteration of frequency than that given by the FitzGerald factor, but even so the experiment was not a feasible one. The spectra of canal rays as then produced consisted of broad diffuse bands, on which measurements of the accuracy demanded were not possible.

When, some years ago, I became convinced of the necessity for some direct experimental proof of the FitzGerald contraction, of a positive character, and looked into the possibility of using canal rays, I found that the situation had materially changed since Stark's suggestion, particularly through the work of A. J. Dempster. Dempster, by accelerating the canal ray particles between two slightly separated perforated plates, between which existed a high difference of potential,

produced canal rays homogeneous in velocity. In place of the broad bands exhibited in the spectroscope by Stark's canal rays, these produced definite lines, sharp enough to be capable of exact measurement as to position. The experiment of determining the change of frequency of an atomic clock in motion thus appeared feasible.

Before describing the experimental apparatus and the experiment as performed, let us look at what is to be expected from the change of clock frequency according to the FitzGerald factor. We consider the Doppler effect. A radiating atom in motion will cause a shift in the wave-length of the light received by a stationary observer in the line of motion. There will be a decrease of wave-length for the observer the particle approaches ; a like increase for the observer from whom the particle is receding. If now the frequency of the particle is decreased, both the received wave-lengths will be increased, and the centre of gravity of the Doppler lines will no longer coincide with the light from a stationary particle. Calculation shows that if the Doppler shift is $\frac{\triangle \lambda}{\lambda}$ then the shift of the centre of gravity of the fore and aft lines will be $\frac{1}{2}\left(\frac{\triangle \lambda}{\lambda}\right)^2$. We will have a "second order Doppler effect." The experimental problem is to produce canal rays of a velocity sufficient to make $\frac{1}{2}\left(\frac{\triangle \lambda}{\lambda}\right)^2$ an accurately measurable quantity. It develops that applied voltages of 10,000 to 50,000 are adequate.

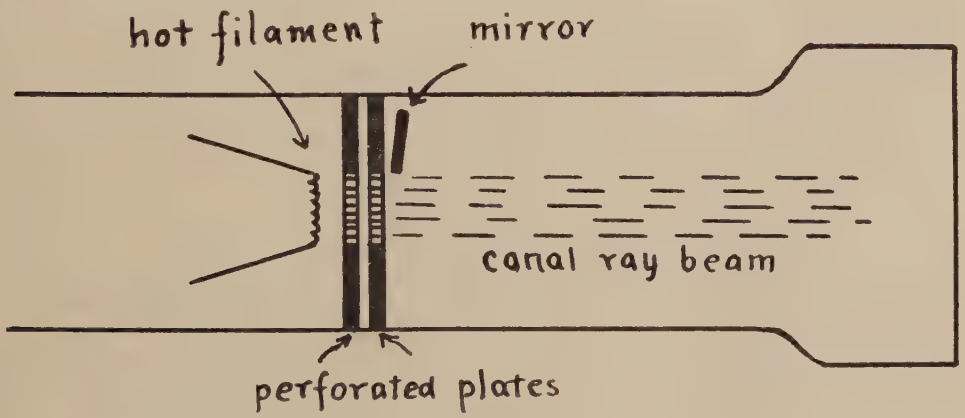


Figure 3

I show first (Fig. 3) a hydrogen canal ray tube of the Dempster type, modified to give the experimental conditions matching the theory as above outlined. Ionized particles are produced by an arc between a hot filament and the first of two slightly separated perforated plates. The ions which diffuse into the space between the plates are subjected to a high impressed voltage and emerge through the perforations

of the second plate as canal rays of high and uniform velocity. In front of the plates, close to the side of the stream of canal rays, is placed a mirror.

When the light from the canal rays is photographed in a spectrograph, we obtain pictures of the type shown in Fig. 4, Pl. 3. In these pictures, which show the sharpness of the lines obtained by the Dempster design of tube, we have a pattern which, until it is carefully measured, appears to consist of a central line, with symmetrically placed lines to either side. The central line is due to the monochromatic light emitted by stationary atoms, which are always present in some number. To either side we have the displaced lines due to the canal rays coming toward the spectroscope, and to the reflected light corresponding to a similar stream in the opposite direction. Note that in this picture we have not a single Doppler line, but two ; these correspond to hydrogen of double and triple mass. The relative intensities of these Doppler lines, and of the central undisplaced line, vary greatly with conditions of pressure and voltage, and in this series of experiments only the H_2 and H_3 lines were of measurable intensity. According to the theory above, we should expect to find the pattern of Fig. 4 not exactly symmetrical, on careful measurement, but in all cases the centre of gravity of two corresponding lines displaced with respect to the central line by the amount $\frac{1}{2}\left(\frac{\triangle\lambda}{\lambda}\right)^2$.

In Fig. 5, Pl. 3, are shown the results of many such measurements, made with several canal ray tubes, on ions of different masses, over a large range of applied voltages, plotted in Angstrom units. The full line shows the values to be expected from the theory. The outcome of the experiment is to confirm the predicted change of frequency, and thereby also, from considerations above given, the FitzGerald contraction of length.

IV.—Discussion.

This survey of the contractions introduced by FitzGerald, which presents them as the fundamental physical implements of the principle of relativity, may be concluded by some observations on the views current in FitzGerald's time on the relations of radiation and matter, and implicit in the formulation of his contraction.

For FitzGerald there existed a medium, responsible for the transmission of radiation with a velocity expressed in terms of a co-ordinate system stationary in this medium—the luminiferous ether. His postulated contraction of matter was in terms of motion with respect to this ether. Since his time it has become fashionable to deny the existence of the ether. This attitude has been bolstered up by the contention that the ether is unnecessary and its existence unproven. The argument that it is unnecessary rests on the claim that the special theory of relativity is developed without the idea of the ether. On examination it will be found that what the special theory of relativity does is to substitute for the ether the so-called

" principle of the constancy of the velocity of light." This " principle " has been reviewed above and found to be untenable ; it is not supported by fact, and is self contradictory. It offers no valid substitute for the ether.

As to proof of the existence of the ether, you will recall that Lorentz, who used the ether as his framework, and never abandoned it, argued that the Michelson-Morley experiment proved the FitzGerald contraction. Now with the ether in question, his argument, re-oriented with the Michelson-Morley experiment and the experimentally established FitzGerald contraction as proved facts, becomes an argument in proof of the ether. Had the ether not been postulated, the Michelson-Morley experiment and the experiment on canal rays would have made it necessary to invent it.

In discussing the question of the ether, we must account not only for the Michelson-Morley experiment, but for the optical phenomena exhibited on rotating bodies. Experiments in this field are those of Sagnac, who sent two beams of light in opposite directions around a rotating platform, and that of Michelson and Gale, who sent two beams of light around a large path having segments of different latitude and velocity on the earth's surface. These experiments gave positive, first order results, accurately described by the supposition that the rotation is with respect to a fixed ether. I suggest for consideration the experiment of placing a Michelson-Morley apparatus anywhere in the long path of the Michelson-Gale apparatus. It would yield a nul result. while the Michelson-Gale apparatus, using the same light beams, would exhibit a positive, first order effect. Both are explained satisfactorily by the ether, and the FitzGerald contraction taking place in the ether.

These speculations and experiments thus do more than confirm the contraction postulated by FitzGerald. They support the physical theory of the relation of matter and radiation of FitzGerald's day, in opposition to the mystical views on space and time which have had a vogue more recently.

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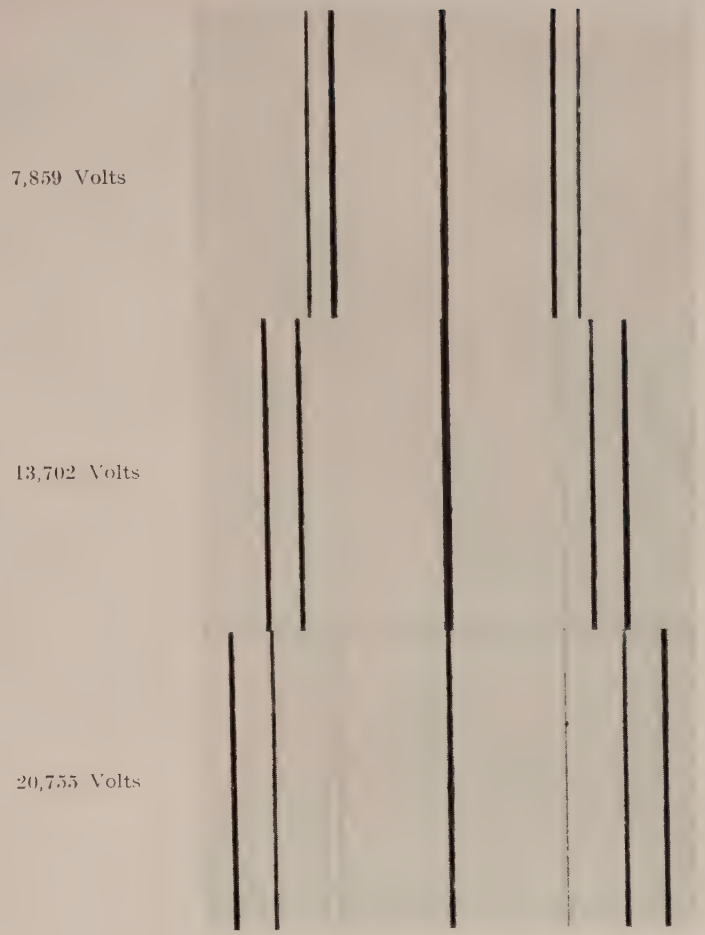

Figure 4

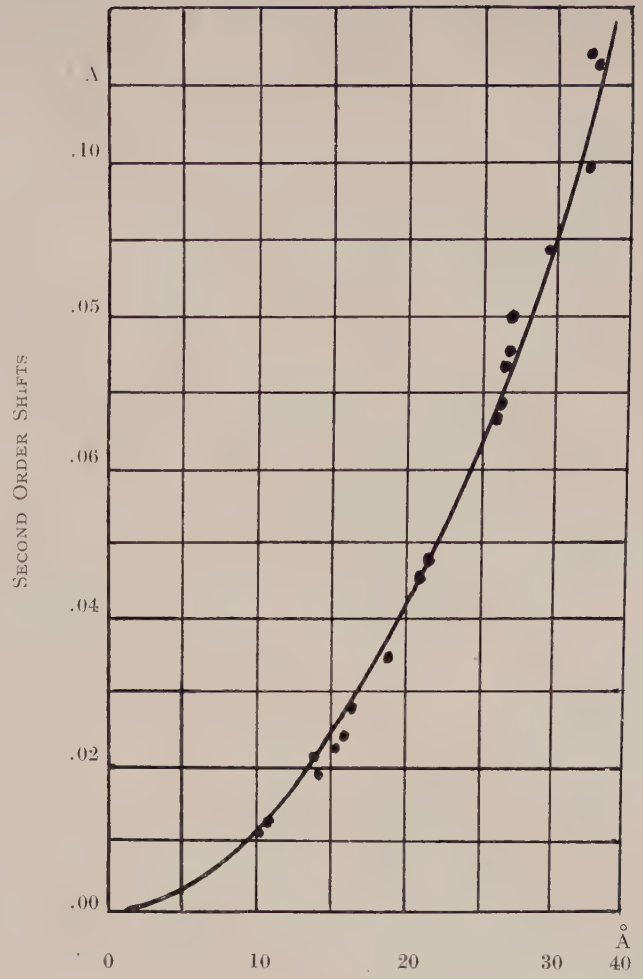

Figure 5

[To face page 16

No. 1.C

THE FITZGERALD-LORENTZ CONTRACTION—SOME PARADOXES AND THEIR RESOLUTION.

By

W. H. McCREA.

I.—Introduction.

So far as the writer is aware FitzGerald himself never published any account of the hypothesis concerning the contraction of moving bodies that now bears his name.[1] However, in view of his contributions to the electrodynamics of moving systems it is likely that he supposed the contraction to be a possible consequence of this theory. Larmor and Lorentz subsequently found that it could in fact be derived in this way. Nevertheless, Einstein's theory of special relativity in due course removed the necessity for any investigations of this sort. By making it depend explicitly only upon transformations of space time, this theory ostensibly rendered the contraction independent of the properties of matter.

That this last inference is fundamentally incorrect is shown by the fact that any observer's assignment of spatial coordinates to any event is related ultimately to a rigid measuring-scale with which he is assumed to be equipped. The reason why explicit mention of the physical properties of rigid bodies does not appear in the theory of special relativity is that this theory is concerned only with frames of reference in which a relatively stationary rigid body is subjected to no physical processes.

All paradoxes concerning special relativity are in some way concerned with cases of non-uniform motion, to which the theory is not in fact strictly applicable. However, when the paradoxical features are eliminated by avoiding illegitimate applications, the theory is usually found to yield some information about the phenomena concerned. Some such cases are considered in the present paper.

The interest of these cases is that they do illustrate the fact that special relativity is inevitably concerned with certain general properties of matter.

[1] His only published mention of the contraction which I have found is in his review of Larmor's *Aether and Matter* (*Scientific Writings* of G. F. FitzGerald (1902) 508), where he gives no indication of his own initiation of the conception.

2. The FitzGerald–Lorentz Contraction.

The effect of the FitzGerald-Lorentz (FL) contraction, stated in general terms, is that a moving body should appear to be contracted in the direction of its motion, the contraction-factor being

$$\alpha=\sqrt{(1-V^2/c^2)} \qquad (2\cdot1)$$

where c is the speed of light and V the speed of the body. It is assumed that $V<c$.

In particular, and more precisely,[2] if say a rigid rod is moving lengthways along a rigid scale S with uniform speed V, it is asserted that at any instant *the rod covers a length* αL of the scale, where L is independent of V. If the rod is at rest, then $V=0$ and $\alpha=1$, showing that L is the length measured in the usual way. We call L the *proper-length* of the rod.

When FitzGerald first proposed his hypothesis of the contraction, he presumably regarded V as the speed relative to an aether which defined a preferential frame of reference. I know of no evidence that he contemplated any principle of relativity other than the Newtonian one. Therefore I do not suppose that he saw any necessity to consider, for example, the point of view of an observer travelling with our rod. Even had he done so, I suppose at that time he would have concluded that such an observer must also regard the rod as covering length αL of S. This would have been on the simple ground that, if the rod is contracted relative to the scale, then presumably the scale is expanded relative to the rod.

However that may be, so long as we take no account of a possible observer moving with the rod, the question as to whether the contraction denotes an *intrinsic* change in the rod does not arise. For any question of intrinsic properties presupposes an observer on the rod in terms of whose observation those properties must be described.

Ever since Einstein's formulation of the theory of special relativity in 1905, if not since the earlier work of Lorentz, the FL contraction has, however, been treated as a deduction from that or an equivalent theory. But according to such a theory, it does not matter whether we consider the rod or the scale as moving. Therefore, since an observer attached to the scale regards the rod as being contracted, an observer attached to the rod must regard the scale as being contracted (*not* expanded). A well known inference is that at any instant in the experience of the latter observer, *the rod covers a stretch of* S *of length* βL *as indicated by the graduations of* S where[3]

$$\beta=1/\alpha. \qquad (2\cdot2)$$

Moreover, since it does not matter whether we regard the rod or the scale as being at rest or in motion (so long as the relative speed is the same) *the theory expressly denies that the contraction denotes any intrinsic change* in the rod, or, of course, in the

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[2]To be still more precise, we ought to specify that the observer and the scale S are at rest in an " inertial frame " F, this term being used in the sense of the theory of special relativity. It is hoped, however, to give a more detailed account of the present work elsewhere. That account will be developed with the explicit statement of such conditions. The reader of the present account may be prepared to take it that the " usual conditions " are assumed to be satisfied.

[3]It is convenient to use the two symbols α, β so as to avoid the frequent occurrence of reciprocals. The reader will keep in mind that $\alpha<1$ and $\beta>1$.

scale. The contraction is purely a result of the way in which observations made by different observers are related, the relations being given in fact by the *Lorentz transformation*.

3. Two Paradoxes.

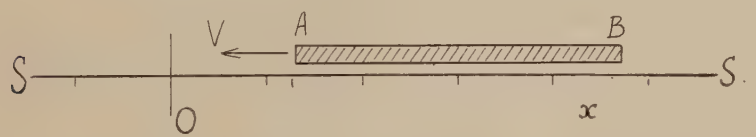
The various statements made in Section 2 invite the formulation of two paradoxes. Each has the form of an apparent inference that the contraction does denote an intrinsic change in the rod. The fact that the two " inferences " contradict each other immediately shows that at least one of them must be a paradox. Or, if we choose, we may present this contradiction as an additional paradox. The " inferences " are :—

I. It would be merely another way of describing the FL contraction to say that at any instant an observer O on the scale could trap the moving rod in a box of length αL. He could then exhibit the rod in a permanently *contracted* state, i.e., as having suffered an intrinsic physical change.

II. It would again be merely another way of describing the FL contraction to say that an observer on the rod could at any instant engage the rod on to the scale S while it is covering a length βL of S. In that case the observer O on the scale could then exhibit the rod in a permanently *expanded* state, i.e., as having again suffered an intrinsic physical change, but of a sort opposite to that in I.

It may well be that these paradoxes were propounded in the days when the FL contraction was still a novel conception. However, I have not seen them stated elsewhere. Their resolution is an instructive, albeit by no means profound, exercise in analysing the meaning of the contraction. At the same time, the resolution gives rise to further considerations having extensive consequences in relativity theory.

4. Applications of the Lorentz Transformation.



Let O be the zero-point of the scale S and let x be distance measured along S from O as shown by the graduations of S. Let t be time as measured by observer O.

In this and other cases it is convenient to use the same letter to denote a point (or more strictly a particle fixed in a body) and an observer at that point (i.e., attached to the particle).

We denote the given rod by AB. We suppose its undisturbed motion to be towards O from the side of positive x. According to O the motion of A is therefore given by

$$x = -Vt, \qquad (4.1).$$

the origin of t being supposed suitably adjusted, so that A will reach O at $t=0$.

Let t' be time as measured by an observer moving with the rod, the origin of t' being also adjusted so that A will reach O at $t'=o$.

In the two time-reckonings, we denote by t_A, t'_A the epochs of the event of the passage of A over the graduation-mark labelled X_A on S. Similarly t_B, t'_B are the epochs of the passage of B over the X_B-graduation.

Simple applications of the Lorentz transformation then yield the formulae :

$$t_A = -X_A/V \quad (4.2)$$
$$t'_A = -\alpha X_A/V \quad (4.3)$$
$$t_B = -X_B/V+\alpha L/V \quad (4.4)$$
$$t'_B = -\alpha X_B/V+L/V \quad (4.5)$$

Equation (4.2) is a repetition of (4.1). The appearance of the factor α in (4.3) is the effect of the phenomenon of " time-dilation," which is also directly deducible from the FL contraction and Einstein's (restricted) principle of relativity.

The stretch $(X_B—X_A)$ of S covered by the rod at any instant $t_A=t_B=t$ in O's experience is from (4.2), (4.4)

$$(X_B—X_A)=\alpha L, \quad (4.6)$$

while the stretch $(X_B—X_A)'$ of S covered by the rod at any instant $t_A'=t_B'=t'$ in the experience of an observer on the rod is from (4.3), (4.5)

$$(X_B—X_A)'=\beta L. \quad (4.7)$$

The results (4.6), (4.7) agree with the assertions of section 2.

5. Observed " Lengths."

Any observer P, say, at any instant T in his own immediate experience *sees* the rod AB as momentarily covering some stretch of the scale S. The length of the stretch as read off the scale S we shall call the *apparent scale-length* of AB for P at T.

Since it is given that the motion of the rod is uniform, any observer such as P can use his observations to *infer* the scale-positions X_A, X_B occupied by A, B at any one and the same instant in his time reckoning. The difference $(X_B—X_A)$ we shall call the *measured scale-length* of AB for P. This is the standard meaning of a measurement of length by a given scale, and it is used explicitly in the above derivation of (4.6), (4.7).

We require to notice the following properties :—

(*a*) The apparent scale-length is in general *not* equal to the measured scale-length. For, owing to the different times of light-travel from A,B to the observer, he does not actually *see* A,B in positions which he regards as simultaneous.

(*b*) Unless an observer is stationary relative to the rod, its apparent scale-length will vary with the epoch of observation.

(*c*) Unless two different observers are instantaneously coincident, the apparent scale-length is in general not the same for both, whether or not they are at relative rest.

(*d*) If the two observers are instantaneously coincident, the apparent scale-length is the same for both, independently of the relative motion. For they both see the rod by the reception of the same set of light-pulses arriving at their common position.

(*e*) For any observer relative to whom the rod is in uniform motion, the measured scale-length is the same at all epochs.

(*f*) For any observer stationary relative to the observer in (*e*) the measured scale-length is the same, since in that case the two observers use the same time-reckoning.

(*g*) For any observer in motion relative to the observer in (*e*) the measured scale-length is in general different, whether or not they are instantaneously coincident. This is because two such observers use different time-reckonings such that positions of A and B which are simultaneous in the reckoning of one are not simultaneous in that of the other.

(*h*) Assertions about the effect of the FL contraction such as those in Section 2, are customarily made in terms of measured scale-lengths and not apparent scale-lengths. This is primarily because we wish to use "length" in some standard accepted sense, but also because properties (*b*), (*c*) as compared with (*e*), (*f*) render the apparent length an inconvenient quantity in which to express general results.

It follows from (*h*) that any simple general assertion about the FL contraction which seems to apply directly to immediately observable effects is likely to be misleading. This will be evident from the following particular results. They are easily derived from (4.2), (4.4), and the appropriate expressions for the relevant times of light travel.

For O, or any other observer on S, so long as he sees the rod to be approaching him, its apparent scale-length is found to be

$$\left(\frac{1+V/c}{1-V/c}\right)^{\frac{1}{2}}L \tag{5.1}$$

By virtue of (*d*), this is also the apparent scale-length for the observer A on the rod itself.

For any observer on S who sees the rod receding from him its apparent scale length is given by changing the sign of V in (5.1) giving

$$\left(\frac{1-V/c}{1+V/c}\right)^{\frac{1}{2}}L \tag{5.2}$$

and by the same argument as before this is also the result for the observer B on the rod.

It follows from (5.1) that O *sees* the rod as expanded, and not contracted.[4] This is because the light reaching him from the further end B leaves B at a stage in B's motion which is earlier than the stage in A's motion at which the light leaves A

[4]It is scarcely necessary to remark that we are not troubled by the difficulty of seeing anything at all when everything is treated as happening in a single dimension! We permit the observer sufficient latitude to *see* what is happening along the line of S.

in order to reach O at the same instant. In fact a purely classical calculation of this effect gives $(1-V/c)^{-1}L$ in place of (5.1), and this agrees with (5.1) to the first order in V/c. Corresponding remarks apply to the apparent contraction denoted by (5.2).

The fact is often overlooked that such classical effects can produce much "better" contractions and expansions than does the FL effect in what is actually *seen*.

Indeed, the only observer attached to the rod for whom the apparent scale-length has the value βL given by the direct FL effect as in section 2 is the observer attached to its midpoint. An observer attached to S *sees* the rod as having the length αL, as in section 2, only at the one instant when he sees himself to be at the midpoint of its apparent scale-length. As follows by applying (5.1), (5.2) to the two halves of the rod this observer, is *not* then opposite the midpoint of the rod.

6. Rigidity.

In classical theory a rigid body is by definition one in which the distance between any two particles of the body is invariant. Therefore, if one particle of the pair is caused to suffer a sudden change of motion with a non-vanishing component along the line joining them, this must produce an instantaneous change of motion of the second particle. Otherwise the mutual distance would be altered. Consequently, a necessary condition for a body to be rigid in the classical sense is that it should transmit interactions between its parts with infinite speed.

According to the theory of special relativity, on the other hand, an event at one place cannot produce an effect at another place by any means which implies the transmission of the effect with a speed exceeding c, the speed of light. Therefore, as is well-known, the theory does not admit the existence of rigid bodies in the classical sense.

We shall therefore now define *a rigid rod as one along which impulses are transmitted with speed* c.

Since our rod is being treated as a one-dimensional body and since we are dealing only with longitudinal motion, no other condition is required. Moreover, since the theory permits the existence of no "more rigid" body of this sort, there is no objection to adopting the term *rigid* in this sense.

7. The Stopping of a Moving Rod.

We now consider what happens if the end A of the moving rod is suddenly stopped and held at O.

The theory contains no objection to our supposing this to happen. Also we need not consider any possibility of the distortion of the scale S as a result of the impact. We could, for example, allow the rod to meet and engage with an exactly similar one moving with the opposite velocity ; in this way the process need have no material connexion with S.

By virtue of what we mean by the rod being rigid, the effect of the impact at A

is transmitted along the rod with the speed c. *The motion of the end B is entirely uninfluenced until the effect reaches it in this way.*

In accordance with the simple theory of elasticity, I shall assume that B is then momentarily brought to rest relative to A. I shall call this event the stopping of B without implying that it is other than momentary.

Now since the impact is transmitted with the speed of light, the result just stated may be expressed by saying simply that B stops precisely when he *sees* A to stop. At an immediately preceding instant he was completely unaware of the impact (either by seeing or feeling it). Therefore at that instant, he saw the rod as having its apparent scale-length given by (5.2). It follows that *when the rod is brought to rest its length is*

$$\left(\frac{1-V/c}{1+V/c}\right)^{\frac{1}{2}} L. \tag{7.1}$$

This result can easily be calculated by considering the details of the motion without any appeal to apparent lengths. But one interest in deriving it as we have done, is to notice that B sees *the rod as being stopped dead with no change in its apparent length.* This would not be the case were the rod not rigid in the present sense, i.e., were the effect of the impact to reach B with less than the speed of light. Although it holds good for only a particular observer, this property does supply another reason for calling the rod a " rigid " one.[5]

Just before he is stopped himself, observer A, on the other hand, sees the rod as covering a length of the scale given by (5.1) ; when he sees B stop he sees the rod as covering a length given by (5.2) or (7.1). This assertion is unaffected by the fact that, in the language of relativity theory, A has changed " his frame of reference " between the observations. Therefore we can say that A sees a compression of the rod taking place. This is true for a general observer, but the apparent amount of the compression varies from one observer to another.

In O's reckoning the stopping of A occurs at $t=o$ and, from (4.4), the stopping of B at $t=t_B$

where

$$t_B = \left(\frac{1-V/c}{1+V/c}\right)^{\frac{1}{2}} \frac{L}{c} . \tag{7.2}$$

Now O could have pre-set devices for closing gates in front of A and behind B at times $t=o, t=t_B$ respectively. He could therefore trap the rod in a configuration of length (7.1) when it would certainly be in an intrinsically modified state. It is to be noted that this modification is of the first order in V/c whereas that proposed in section 3 is only of the second order.

A corresponding set of considerations apply to the case in which the end B of the rod is suddenly stopped and held. In this case the end A must run on until the effect of the impact transmitted with speed c has overtaken A. Here, *when the rod*

[5]It might be objected that a " real " change has occurred without B seeing it happening. This objection has not, however, the same force as the one to be considered in section 8 (e) below. From here B is transferred from one frame of reference to another by the act of stopping and he must take this fact into account in interpreting his observations.

is brought to rest its length is

$$\left(\frac{1+V/c}{1-V/c}\right)^{\frac{1}{2}}L \tag{7·3}$$

with corresponding changes in the other results.

It must be emphasized that (7·1), (7·3) are the lengths attained momentarily; unless means are employed deliberately to secure the rod in such configurations, it will then proceed to regain its proper length.

8. Resolution of the Paradoxes.

It will be seen that the resolution of paradoxes I, II is contained in section 7. It can now be seen, moreover, that even larger intrinsic changes in the rod than those claimed in I, II can be brought about by operations similar to the ones contemplated in the formulations of I, II.

The following are the relevant considerations :—

(*a*) So long as there is no interference with the uniform motion of the rod, the differences between the various measured and apparent lengths, on the one hand, and the proper length on the other, denote no intrinsic change. An intrinsic change *is* achieved in the manner suggested, but this is produced entirely by the physical process of stopping the motion.

(*b*) The reason a physical change does occur is that the rod cannot be rigid in the classical sense. If one end is stopped the effect does not instantaneously reach the other which must go on moving until the effect does reach it, a " real " contraction or expansion being thereby achieved.

(*c*) In I, if O has arranged for gates to close when $t=o$ at $x=o$ and $x=\alpha L$, he does certainly trap the rod. But, in fact, the gate at $x=\alpha L$ has no immediate effect since the rod will contract still further in accordance with the results of section 7.

In II, if A,B have arranged to hook their ends on to the scale S at time $t'=o$, they will in fact bring the rod to rest with length βL. But if A delays his action until the right moment, they can in fact bring it to rest with the still greater length given by (7·3).

(*d*) With regard to the first part of (*c*), we check from (4.4) that B is in fact passing $x=\alpha L$, at the instant $t=o$ in O's reckoning, and this is the instant at which O stops the end A. It follows that if his action in stopping A were to produce a change in the motion of B before it reaches $x=\alpha L$, his action would have an effect before he performs it! This appears to be a convincing argument that " real " contraction must occur to at any rate length αL. However, such arguments require careful examination and this will not be attempted here.

(*e*) The statement of I suggests that O could assert at one moment that the length of the rod is " really " L though it appears to be αL on account of the FL contraction, and a moment later, after closing his trap, that the length is really αL. This would imply that a real change takes place in the rod without O seeing it happening.

The answer is that the statement is misleading because it is expressed in terms of measured scale-lengths instead of apparent scale-lengths. The latter are required to express what O *sees*. The discussion in section 7 shows that he must see the real contraction actually occurring. Corresponding remarks apply to the statement of II.

9. Modulus of Elasticity.

We assume the rod to be uniform and composed of material of proper-mass m per unit proper-length and modulus of elasticity λ. According to both classical and relativistic mechanics the speed of longitudinal waves of small amplitude is then v where

$$v^2=\lambda/m \tag{9.1}$$

Since there is no dispersion, this is also the speed at which impulses of small amplitude traverse the rod. We have seen that relativity theory requires $v\leqslant c$ and hence we must have

$$\lambda\leqslant mc^2 \tag{9.2}$$

Thus relativity theory imposes an upper limit mc^2 upon the modulus λ and, according to our definition in section 6 this corresponds to the case of relativistic rigidity.[6]

Now the modulus λ has its standard meaning only for small extensions (or compressions), and so in this section we restrict ourselves to these.

We recall in particular that a change of length $\triangle L$ results in an increase of elastic energy

$$\frac{\lambda}{2m}\left(\frac{\triangle L}{L}\right)^2 \text{ per unit mass.} \tag{9.3}$$

The compression (7.1) and extension (7.3) both give to the first order in V/c

$$\triangle L = VL/c \tag{9.4}$$

and so from (9.3) require an increase of energy

$$\lambda V^2/2\,mc^2 \text{ per unit mass.} \tag{9.5}$$

In the experiments considered in section 7, this energy can be supplied only at the expense of the kinetic energy possessed by the rod before it is stopped and this is

$$V^2/2 \text{ per unit mass.} \tag{9.6}$$

In (9.6) we use the first approximation to the relativistic value, i.e., merely the classical value, because our restriction to small disturbances implies restriction to small values of V/c.

Since the energy (9.6) must be at least sufficient to supply the amount (9.5) we require

$$V^2\geqslant 2\lambda V^2/2mc^2 \tag{9.7}$$

From (9.7) we have

$$\lambda\leqslant mc^2$$

which is the same (9.2). Also, if we take $\lambda=mc^2$ we see that the kinetic energy is just sufficient to supply the minimum compression or extension (7.1), (7.3) for the *rigid* rod. Moreover, since this is the case and there is therefore no dissipation of energy, our *rigid rod must be regarded as being also perfectly elastic.*

[6]The quantity λ/m for steel is about 3×10^{11}, while c^2 is nearly 10^{21}. Thus it is seen that the relativistic upper limit is not even remotely of any practical significance.

Were our kinematic considerations in the earlier section in disagreement with the dynamical ones of this section there would, of course, be something radically wrong. Nevertheless, the neatness with which they do fit together is a matter of considerable interest.

As stated, we are here working only to a first approximation in order to use the modulus λ with its standard meaning. However, it is interesting to note that if the tension in the rod, instead of the standard classical form

$$T = \lambda(L_1 - L)/L \qquad (9.8)$$

where L_1 is the stretched length $L + \triangle L$, is given by

$$T = \tfrac{1}{2}\lambda[1 - (L/L_1)^2], \qquad (9.9)$$

then it can be verified that the energy considerations remain satisfied with relativistic forms for all values of V/c, the case of " rigidity " again being given by $\lambda = mc^2$. It is seen that (9.8), (9.9) agree to the first order in $\triangle L$ and so (9.9) leads also to (9.1) for small amplitudes ; I have not yet considered the wave-problem for finite amplitudes.

It should be remarked that it is not legitimate to express λ in terms of Young's modulus E and then to conclude that the inequality resulting from (9.2) must apply to any isotropic material. For special relativity theory as here applied to a one-dimensional body takes no cognizance of our concept of continuous media in general. Relativistic considerations regarding these would have to be developed on their own account.

One of the best-known " paradoxes " of relativity theory is that of the rotating disk. The comment just made shows why the work of the present paper does not immediately help with the solution of this problem. However, the work can be applied to the case of a rotating hoop, as it is intended to show elsewhere.

SUMMARY.

The FitzGerald-Lorentz contraction does not denote any intrinsic physical change in a moving body. Nevertheless it appears that the contracted body could be " trapped " in such a way that such a physical change must result. If in accordance with relativity theory, the speed of transmission of impulses through the body cannot exceed the speed of light, it is shown how the act of stopping the motion will in general produce this physical change. The change may in fact be of a larger order than that inferred from a simple consideration of the FitzGerald effect. These consequences naturally demand a definition of rigidity in relativity theory different from the classical one. It is shown that the definition adopted from the consideration of kinematic effects leads to results consistent with the dynamics of the processes involved. It has to be pointed out that the FitzGerald effect does not directly describe what an observer actually sees: discussion of this circumstance gives additional insight into the problems here treated.

No. 1.D

THE FITZGERALD–LORENTZ CONTRACTION PHENOMENON AND THEORIES OF THE RELATIVITY OF GALILEAN FRAMES.

By

G. J. WHITROW.

"The laws of motions and the effects, and theorems containing the proportions and calculations of the same for the different configurations of the paths, likewise for accelerations and different directions, and for mediums resisting in greater or less degree, all these hold without bringing absolute motion into account. As is plain from this that since according to the principles of those who introduce absolute motion we cannot know by any indication whether the whole frame of things is at rest, or is moved uniformly in a direction, clearly we cannot know the absolute motion of any body."(1)

Bishop Berkeley in his essay *De Motu*, first published in 1721, endeavoured to refute Newton's argument for introducing the idea of absolute motion into natural philosophy. This idea, as he pointed out, cannot be divorced from the idea of absolute space, and in his onslaught on this concept he made the penetrating comment: "It must be admitted that in this matter we are in the grip of serious prejudices."

To succeed in breaking through the hard crust of any fundamental and universal prejudice is the prerogative of genius. George Francis FitzGerald, some one and three-quarter centuries after the publication of his fellow Irishman's study of the nature of motion, achieved international renown by introducing into physics the idea that the length of a rigid rod is not an absolute property of the rod but depends on its motion. This was the first breach in the fundamental assumptions of classical physics. Coming before the discovery of X-rays by Röntgen in 1895, of radioactivity by Becquerel in 1896, and of the electron by J. J. Thomson in 1897, it may justly be claimed that the FitzGerald contraction was the first of the post-classical concepts of modern physics.

Implicit in classical physics was the assumption that a rigid body "possesses" its own spatial measurements. Space itself was supposed to be unique and to possess the properties of Euclidean geometry. The "fact" that it was absolute had been demonstrated by experiments on, and observations of, bodies in rotation, in particular by Newton with his rotating bucket in the seventeenth century and by Foucault with his pendulum in the nineteenth. The basic discipline of physics, however, namely dynamics, was based on the principle of the relativity of Galilean

frames. According to this principle, all Euclidean frames in uniform motion relative to any particular frame of the Galilean class were regarded as equivalent, or " equally good," frames of reference for the fundamental Newtonian laws of motion. This principle was confirmed directly by the apparent failure of all mechanical attempts to detect absolute rectilinear motion, and indirectly by the apparently fool-proof system of dynamics based upon it. Nevertheless, it attributed to space a relativistic character which was difficult to reconcile completely with its absolute nature explicitly postulated by Newton.

The idea of absolute space would seem to imply that in some way the Galilean frames are not equivalent in *all* respects and that, to use an obvious terminology, those at rest are to be distinguished from those in motion, because the absolute space itself must be the continuum formed by the point-origins of the former. Newton, however, was quite unable to isolate this sub-class from the whole class of Galilean frames. Instead, he had to fall back on what Hermann Weyl has called " a hypothesis unfounded in experience and a dialectical dodge which strikes a discordant note in the midst of the magnificent and cogent inductive development of his system of the world in the third book of the *Principia*."(2) This hypothesis concerned "the centre of the world". In the universe of Ptolemy and of Dante there was no need to invoke any special hypothesis, for the centre of the world was placed automatically at the centre of the Earth. In the Copernican universe this centre was placed at the Sun, but once universal gravitation was postulated then the Copernican solution was no longer strictly valid, for the Sun itself must move. Newton argued thus : " Book III, Hypothesis I : That the centre of the system of the world is immovable. This is acknowledged by all, while some contend that the Earth, others that the Sun, is fixed in that centre. Let us see what may from hence follow. Proposition XI, Theorem XI : That the common centre of gravity of the Earth, the Sun and all the planets is immovable. For (by cor. IV of the Laws) that centre is either at rest, or moves uniformly in a right line ; but if that centre moved, the centre of the world would move also against the hypothesis." The next proposition established " That the Sun is agitated by a continual motion, but never recedes far from the common centre of gravity of all the planets." In accordance with the law of gravitation, the Sun like the planets must be acted on by forces and so must move, although only 'a little'. Having established this, Newton then argued : " Hence the common centre of gravity of the Earth, the Sun and all the planets is to be esteemed the centre of the world ; for since the Earth, the Sun and all the planets gravitate one towards another, and are therefore, according to their powers of gravity, in continual agitation, as the Laws of Motion require, it is plain that their movable centres cannot be taken for the immovable centre of the world. If that body were to be placed in the centre towards which other bodies gravitate most (according to common opinion), that privilege ought to be allowed to the Sun ; but since the Sun itself is moved, a fixed point is to be chosen from which the centre of the Sun recedes least, and from which it would recede yet less if the body of the Sun were denser and greater, and therefore less apt to be moved."(3)

In other words, Newton was reduced to *assuming* that the " universe," i.e.,

absolute space, must have a unique centre, that this centre must be at rest and that it is at the centre of gravity of the solar system.

A century later a new epoch in the history of astronomy dawned when, after two hundred failures, a German musician living in Bath at last succeeded in perfecting a more powerful telescope than any before. William Herschel devised this instrument with the object of studying celestial objects outside the solar system. In particular, as a result of his investigations and of those of his successors it became clear that relative to the system of the Milky Way neither the Sun nor even the centre of gravity of the solar system is at rest. By 1927 evidence had accumulated that, apart from any peculiar " local " motion, the Sun, in common with the other stars, forms a vast system which rotates about a centre in the direction of the great star cloud in Sagittarius.

Meanwhile, the problem of whether motion is absolute or relative had ceased to be purely a problem of mechanics, i.e., of material motion. The study of radiation, notably by Clerk Maxwell, had yielded a new " constant of nature," namely the velocity of light *in vacuo*. With respect to which frames must this velocity be measured ? According to the traditional kinematics no *finite* velocity can be invariant with respect to *all* Galilean frames, but only with respect to those frames which are at rest relative to a given Galilean frame. Consequently, Clerk Maxwell's velocity constant c would appear to isolate a sub-class of Galilean frames forming an absolute space. This absolute space, however, was no longer the Newtonian void against which Berkeley directed his polemical attack : " It seems therefore to be mere nothing." Instead, it now became a peculiar new physical entity, the luminiferous ether. With respect to this ether all motion could be regarded as " absolute," and consequently Michelson and Morley performed their famous optical experiment to determine the absolute motion of the Earth. Their failure indicated that the velocity of light *in vacuo* must be regarded as invariant for *all* Galilean frames, a result which could not be reconciled both with the traditional kinematics and with the classical rigid body concept.

FitzGerald was the first to have an inkling of this revolutionary conclusion, and in effect to point out that if the traditional kinematics were to be preserved then the classical rigid body concept must be modified. He postulated that length measured in the direction of motion is a function of velocity, so that if l_0 denotes its length when a body is at rest and l when it is moving with speed V then

$$l=l_0\sqrt{(1-V^2/c^2)}.$$

The FitzGerald contraction phenomenon, later suggested independently by Lorentz, was an *ad hoc* physical postulate introduced to save the traditional kinematics in the face of the null-result of the Michelson-Morley experiment. Nevertheless, it ultimately entailed two consequences of the greatest significance for modern physics :— (i) the conception of a finite upper limit c for all material velocities, and (ii) the idea that the length of a rigid body is not an intrinsic property of the body, but instead is a particular relation between the body concerned and the observer.

The unsatisfactory feature of FitzGerald's hypothesis was the implicit divorce between physical measurement and mathematical transformation. According to

Newtonian kinematics, if $\triangle x$ is the distance between two points on the x-axis of the frame $Oxyz$ and $\triangle x'$ the distance between the same two points regarded as points on the x' axis of the frame $O'x'y'z'$, the second frame moving with uniform velocity V along Ox, then $\triangle x' = \triangle x$. But, according to FitzGerald, a material measuring rod carried by O' along Ox would suffer an apparent contraction, compared with a similar rod carried by O, concerning which the Newtonian kinematics is silent. It is well-known that the physical and mathematical aspects of the problem were completely reconciled in 1905 by Einstein, who postulated the invariance of the peculiar velocity c with respect to all Galilean frames, and the complete equivalence of these frames for the description of radiation phenomena equally with mechanical. The FitzGerald-Lorentz contraction phenomenon then reappeared as an immediate consequence of a new kinematics which automatically took account of the " behaviour " of measuring rods.

Einstein's theory of Special Relativity, however, threw no light on the problem of absolute rotation. Indeed it accentuated the problem, for it made the luminiferous ether, if not superfluous in all respects, at least irrelevant for problems concerning motion. Although it appeared that radiation equally with matter conspired to defeat any attempt to detect absolute rectilinear motion, the same mechanical criteria still existed as before for detecting absolute rotational motion. At the same time there was no clear criterion for isolating the class of Galilean frames[1] from the totality of all imaginable frames. So long as physical science was regarded as the *description* of the external physical world, the absence of such a criterion was a serious philosophical difficulty.

A famous attempt to overcome these difficulties was made by Mach. " Volkmann," he wrote, " advocates an *absolute* orientation by means of the ether . . . but I am extremely curious to know how one ether particle is to be distinguished from another. Until some means of distinguishing these particles is found, it will be preferable to abide by the fixed stars, and where these forsake us to confess that the true means of orientation is still to be found."(4)

We have already noted, however, that the system of fixed stars, the Milky Way, is now thought to rotate. Consequently, it might be thought that the centre of this rotation and a frame rigidly attached to this centre should be considered as the ultimate " centre of the world " and the basic Galilean frame. But in 1924, Hubble at last discovered a crucial test, depending on the identification of Cepheid variable stars, for establishing the existence outside the Milky Way of vast comparable stellar systems. Five years later he announced that, if the peculiar spectral shifts observed in these systems are Doppler shifts, then these systems are all receding from our own with velocities proportional to their respective distances. Conse-

[1]Although this brief account does not pretend to do justice to the history of this fundamental problem, mention should be made of C. Neumann's inaugural lecture at Leipzig on 3rd November, 1869, " Ueber die Principien der Galilei Newton'schen Theorie " (Leipzig, 1870), in which he introduced his famous postulate of the physical existence of a basic rigid body ' alpha,' so as to give an objective meaning to Newton's first law of motion and the class of inertial frames. Neumann's concept may be associated with Gödel's " compass of inertia " (8). Incidentally, according to W. Wilson (*Science Progress*, 38, 622, 1950), Neumann was perhaps the first to enunciate clearly the principle of Newtonian relativity.

quently, by 1930, it was becoming evident that Mach's " fixed stars " would have to be replaced by the framework of the expanding universe of extragalactic nebulae. This conclusion was developed most fully by E. A. Milne, whose sudden death overshadowed the special meeting of the Royal Astronomical Society convened in Dublin in September, 1950.

According to the ideas developed by Milne, the system of galaxies approximates closely to a continuum of " fundamental particles " the relative orientations of which are invariable. (5). Each particle is the origin of a fundamental equivalent frame of reference, and the whole system forms a basic framework with respect to which we can speak of rotation as absolute. At each point of space, to use familiar terminology, there is one and only one such particle which, therefore, constitutes a local absolute standard of rest. The complete class of Galilean frames is formed by these fundamental particles together with all frames moving uniformly with respect to them, according to a particular system of measurement devised so that the fundamental particles themselves appear to be relatively stationary. Thus Milne evaded the difficulties of previous theories concerning the isolation of Galilean frames and the existence of absolute rotation ; but in so doing he was compelled to introduce a non-Doppler interpretation of the red-shifts in the spectra of the external galaxies, and, more germane to our present theme, *a novel distinction between fundamental and subsidiary Galilean frames.*

In one of his last papers published in 1950, Milne attempted to exploit this distinction to the full, and claimed that the laws of transformation from the space-time frame of a fundamental Galilean observer to that of a subsidiary Galilean observer, and from the frame of one such subsidiary observer to that of another, are not of the familiar Lorentz type, although the Einstein velocity-addition formulae still hold good, and " are thus seen to be more general than the Lorentz formulae themselves."(6) Milne's formulae, replacing the Lorentz formulae, introduce the *absolute* velocity of a subsidiary Galilean frame, namely its velocity with respect to the system of fundamental frames, as well as the *relative* velocity of two observers. If unprimed symbols relate to a fundamental frame A and primed to a subsidiary one B moving in the direction of the x-axis of the former with velocity V, Milne's formulae connecting them are of the type

$$x' = \frac{x - Vt}{1 - V^2/c^2}, \quad t' = \frac{t - Vx/c^2}{1 - V^2/c^2},$$

whence

$$x = x' + Vt', \quad t = t' + Vx'/c^2,$$

there being no longer symmetry between A and B. It follows that, if l' denotes the length according to B of a rigid rod carried by B in the direction of B's motion and l its length according to A, then

$$l = l' \ (1 - V^2/c^2),$$

so that A regards B's rod as contracted *by the square of the FitzGerald contraction factor.* On the other hand, B will assign no contraction to the length of a rigid rod carried by A in the same direction. The null-result of the Michelson-Morley experi-

ment is explained automatically, as in the Special Theory of Relativity, since the velocity of light still takes the same value for *all* Galilean observers. Also Fizeau's experiment, etc., are explained in the same way as before, since the Einstein velocity-addition formulae still hold good.

Nevertheless, Milne's formulae are not entirely free from objection. They are based on the convention that the moving observers " pick up " their time from the fundamental stationary observers, in other words that at each instant B's clock agrees with the clock of the fundamental observer (stationary relative to A) whom it is passing at that instant. We have no reason, however, for assuming, without considerably more detailed investigation than Milne undertook, that this is adequate for our study of *natural* clocks, e.g., atomic clocks, life-times of mesons, etc., although of course, it is a perfectly valid postulate to invoke for the mathematical analysis of appropriately defined *conceptual* clocks.

A further practical difficulty in applying Milne's formulae concerns the explicit introduction of the absolute velocities of subsidiary Galilean frames. The precise identification of the fundamental frames *in practice*, and hence the determination of the absolute velocities, i.e., velocities relative to these frames, of the subsidiary Galilean frames is a problem of forbidding complexity. Milne himself used to speak of the fundamental frames as if they were associated with the cores or centres of the extragalactic nebulae, but such an identification applies only to a " smoothed-out " model of the universe. In the actual universe these nebulae form clusters, have their own " proper motions " and, as Baade and Spitzer have recently argued, sometimes collide and pass through each other without losing their respective identities (7), so that their centres are certainly *not* distributed in the same way as a continuum of geometrical points, which is the manner of distribution of Milne's fundamental observers.

Moreover, Gödel has recently constructed a new type of cosmological solutions of Einstein's field equations of gravitation in which the compass of inertia everywhere rotates relative to matter (8). He argues that the mathematical existence of these solutions suggests the *possibility* that in the physical universe, unlike Milne's, the compass of inertia may conceivably rotate relative to the totality of galactic systems.

These considerations, I believe, strengthen the case for a philosophy of science which regards the laws of nature as tools for the investigation of the physical world rather than as descriptions of " objective reality " (9). From this point of view we can regard the problem of Galilean frames as a *conventional* one, with the proviso that the convention to be adopted is not merely one of theoretical choice but also of *practical convenience*. Thus, for problems in which the rotation of the Earth can be neglected it is convenient to take a point fixed on the surface of the Earth as the origin of a Galilean frame ; for problems in which the rotation of the Earth cannot be neglected we may find it convenient to take the centre of the Earth ; and for problems in which we cannot neglect the Earth's orbital revolution we may take the centre of the Sun, or better, like Newton, the centre of mass of the Solar system. Working backwards, we can argue that for appropriately short intervals of time the

motion of the centre of the Earth relative to that of the Sun, and of a point on the Earth's surface relative to its centre can each be regarded as effectively uniform. Similarly, unless we are obliged to consider intervals of time running into millions of years, we can regard the centre of mass of the Solar system as the origin of a Galilean frame moving uniformly relative to the centre of the Milky Way, which itself may be regarded for most purposes as moving uniformly with respect to some more fundamental frame of reference. At each stage we can neglect the actual uniform motion relating it to the succeeding stage.

Turning now to Milne's analysis of Galilean frames, we note that his convention —a purely theoretical one—by which the clocks attached to the subsidiary frames " pick-up " the time kept by the clocks attached to the fundamental frames, results in the explicit appearance of absolute velocities in the transformation formulae. We can, however, avoid this situation provided that we modify Milne's convention appropriately. The necessary modification is *unique*, and it consists in replacing his convention by the rule that a clock moving with velocity V relative to the fundamental frame picks up time $\tau\sqrt{(1-V^2/c^2)}$ instead of time τ. We then find that absolute velocities no longer appear explicitly, and that all Milne's formulae transform into formulae of Einstein-Lorentz type. (Incidentally, the formal results of Milne's analysis can be obtained automatically, by taking the latter formulae and replacing the times t' of " moving " clocks by $t'(1-V^2/c^2)^{-\frac{1}{2}}$, etc.. where V denotes the relative velocity). In particular, by adopting our convention, which is no more arbitrary than Milne's, we obtain the original FitzGerald-Lorentz contraction formula.

Nevertheless, despite formal mathematical similarity, we recapture these well-known results with a difference. No longer do they imply a background of " empty space," against which it is difficult to picture absolute rotation and to isolate the class of Galilean frames. Instead, we can now associate these formulae, regarded as tools of proved practical value for the investigation of physical phenomena, with the conception of a non-empty background formed by a large-scale distribution of matter approximating to that in the actual universe. Against this background we can immediately isolate the class of Galilean frames and assign an unequivocal meaning to absolute rotation, independently of Gödel's suggestion concerning the compass of inertia. Although by no means the last word has been said on the subject, I believe that it is along lines such as these that we shall be able to reconcile most of the familiar formulae and results of pre-cosmological physics with our modern ideas of the world as a whole.

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Summary.

In order to explain phenomena of rotation Newton postulated absolute space. Classical dynamics, however, was based on the assumption of the relativity of uniform motion, although no criterion was provided for isolating Galilean frames from all possible frames in empty space. In the nineteenth century, physicists replaced the concept of empty space by that of the luminiferous ether. To account for the failure of attempts to measure the velocity of the Earth relative to the ether, Fitz-Gerald, and later Lorentz, suggested that the length of a material measuring rod is a particular function of its motion. This suggestion was shown by Einstein to be an immediate consequence of his Special Theory of Relativity, which combined the postulate of relativity of uniform motion with the invariance of the velocity of light for all Galilean frames. This theory replaced the ether by empty space once more and so provided no criterion for isolating the Galilean frames and no explanation of rotational phenomena.

Solutions of these problems were given by E. A. Milne, who based his theory of world-structure on the concept of a continuum of fundamental particles which forms a simplified model of the " smoothed-out " system of extragalactic nebulae. This theory entails a novel distinction between fundamental and subsidiary Galilean frames. Milne derived new formulae to replace the Lorentz formulae and a new formula for the FitzGerald-Lorentz contraction. Milne's formulae, while in accord with the null-result of the Michelson-Morley experiment, introduce the absolute velocities of subsidiary Galilean frames. It is now suggested that this is an unsatisfactory feature of Milne's analysis which can be avoided by a unique modification of his arbitrarily chosen postulate concerning the clocks attached to moving frames. The result is to combine the advantages of the rival theories of Einstein and Milne whilst eliminating the respective shortcomings of each. In particular, the usual formula for the FitzGerald-Lorentz contraction phenomenon is restored.

No. 1.E

EFFECTS OF ACCELERATION IN THE MICHELSON-MORLEY EXPERIMENT

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1. The Observations of D. C. Miller.

In 1933 D. C. Miller[1] published an analysis of his observations of fringe shifts obtained in the repetition by him of the famous Michelson-Morley experiment of 1887, using a very sensitive interferometer. He insisted that the null result described in books on relativity was not in agreement with the observations of Michelson and Morley nor with his own, and that, although the fringe shifts observed were much smaller than what was to be expected on the assumption of a fixed ether, they lay within the range of observability, and constituted a physical fact demanding explanation. He himself supplied such an explanation in terms of a fixed ether.

It is a pity that Miller attempted this explanation of his observations. One is forced to reject it ; it conflicts with the special theory of relativity, now well absorbed into the body of physics, and it also contains an arbitrary procedure very hard to accept. But it would be very unscientific to reject as false a record of observations because this record is accompanied by an unacceptable explanation. The history of science is full of false explanations, and it is only when a physicist allows his false explanations to get mixed into his observations that any real damage is done. In Miller's paper this does not occur to any serious extent, and one can easily reconstruct from statements about velocity through the ether valid statements about fringe shifts actually observed.

Beside the record of Miller's observations we must set the results obtained by Joos[2], Kennedy[3], Illingworth[4], and Michelson (with collaborators)[5]. These experimenters repeated the Michelson-Morley experiment with various refinements and special devices, so that their interferometers were more sensitive than Miller's instrument. In no case did they report an observed fringe shift comparable to those seen by Miller ; in fact, we may say that for them the effect was null.

This extraordinary situation in relation to one of the most fundamental experiments of physics is a challenge to the theoretical physicist. Since Miller's observations

appear to be in disagreement with the special theory of relativity, and the null result obtained by the others to be in agreement with it, there is a temptation to brush aside Miller's results as due to some systematic error. But physics is not made in this way. A systematic error capable of producing those trends which appear strongly in Miller's results would in itself be worthy of investigation. The challenge to explain theoretically not only the results of Miller but also the null results of the other experimenters remains. The present paper offers an explanation. The explanation may appear fantastic, but it is not on such grounds that theories should be judged. The theory admits a direct experimental test. If it should succeed under that test, the puzzle may be regarded as solved ; if the theory fails under test, the puzzle remains, and the responsibility of finding an explanation falls back on all theoretical physicists interested in such matters.

2. What is the Michelson-Morley Experiment?

There are two quite different ways of describing any experiment in physics—the theorist's way and the experimenter's way—and the Michelson-Morley experiment is no exception. The theorist's description is to be found in any book on relativity. According to this description, a rigid body carrying a source of light and mirrors is at rest in some Galileian (unaccelerated) frame of reference, and interference fringes are formed by light travelling along two different paths. The body is turned and fringe shifts (if any) observed. The theory of relativity tells us that under these circumstances no fringe shift will occur (null result).

The present paper is written with full acceptance of the special theory of relativity. That means that the above statement is accepted—a null result for the Michelson-Morley experiment performed as just described.

But no one ever has carried out such an experiment. A Galileian frame of reference is not available ; as far as the Michelson-Morley experiment is concerned it is a fiction of the theorist's mind, and the experimenter has nothing to do with it. Instead of a fictitious rigid body he takes a slab of stone or a metal structure, and instead of a fictitious Galileian frame of reference he uses the rotating earth.

Such discrepancies between theory and practice occur in every experiment and we are not to be shocked by them. The gap between theory and practice is as a rule to be bridged by common sense and not peered into. But certain occasions arise when it is necessary to peer into the gap, and this is one of them. We must insist that the experiments performed under the general name of " Michelson-Morley experiments " have not been Michelson-Morley experiments in the theoretical sense, and that we have no right to make any prediction at all about the results of the experiments as actually performed until we have created a theory adequate to cover the fact that the interferometer is carried on the rotating earth. It is true that the acceleration resulting from the earth's rotation is small, but in experiments of such refinement we must not be rash ; until we have made a theory of rigid bodies in accelerated motion, we are not in a position to decide what accelerations are negligible.

3. The Rigid Body in Relativity.

The word " rigid " is commonly applied to such things as pieces of stone or metal, and by this we mean that the motions of such bodies agree closely with the motions of ideal rigid bodies, mathematically defined, but not thought of as physically existent. Wherever confusion is likely, we may refer to " physically rigid bodies " (actually existing) and " mathematical rigid bodies " (existing only in the minds of mathematicians). The plan, for both Newtonian and relativistic physics, is to construct a theory of mathematical rigid bodies and test its physical validity by experiments performed on physical rigid bodies. We cannot handle the former nor reason about the latter.

In Newtonian physics the definition of a mathematical rigid body is clear and simple—the distance between any two of its particles remains constant. We cannot take this definition directly over into relativity because the measurement of distance involves the specification of a frame of reference in which the measurement is to be done, and the absolute frame of Newtonian mechanics is not available in relativity. But if we could succeed in specifying a frame of reference in a relativistic way we could then take over the Newtonian definition into relativity ; however we would have no assurance, without experiment, that the behaviour of a mathematical rigid body, so defined, would be reproduced by the behaviour of a stone or piece of steel. That is just one way of creating a definition of a relativistic rigid body. There are others, and one must realise at the outset that there is no unique definition awaiting our discovery of it, but many possibilities with various degrees of simplicity and plausibility. We do not seek a physical reality (it is only a mathematical concept), and of the many possibilities we must pick the one that serves best as a model on which to base predictions.

Von Laue[6] has given reasons why a rigid body with only six degrees of freedom is a concept which violates the principle of relativity. But if we admit this and as a result refrain from defining such a body, then any theoretical description of the Michelson-Morley experiment must involve the elastic properties of the materials used. This is contrary to the view commonly held that the results of the experiment are independent of the materials, provided they are physically rigid bodies. Indeed, after reading von Laue's rejection of the concept of the rigid body, one turns with some curiosity to find how he avoids this dilemma in the description of the Michelson-Morley experiment given in his book (*op. cit.*, p. 28). He avoids it by omitting to mention what the arms of the interferometer are made of. Silberstein[7] is more realistic ; he writes " . . . they made the configuration . . . ' rigid ', that is to say as rigid as a stone is."

Born[8] and Herglotz[9] have defined a relativistic rigid body, using what seems to be the most natural extension of the Newtonian idea. Their definition has been discussed by Silberstein (*op. cit.*, p. 204) and recently by Fokker[10]. But the Born-Herglotz rigid body does not satisfy our needs, since it has in general only three degrees of freedom, instead of the required six.

If we now approach with an open mind the question of defining a relativistic

rigid body, the first criterion is that the definition should not differ much from the Newtonian definition in all practical cases. The second is that the definition must explain why the Michelson-Morley experiment did not give the comparatively large fringe shift expected on the basis of a fixed ether. This fact has now been so closely woven into the structure of the theory of relativity that it is well to state the basic hypothesis in terms of the behaviour of a rigid body, as FitzGerald did.

The FitzGerald contraction hypothesis runs as follows : Two equal rigid bars, A and B, lie together at rest in a Galileian reference frame S. A remains at rest and an acceleration is given to B, its velocity being ultimately made constant with magnitude v in the direction of A. Then the length of B, measured by simultaneous observations in S, bears to the length of A the ratio $(1-v^2/c^2)^{\frac{1}{2}}:1$, c being the velocity of light. There is no lateral contraction.

This hypothesis implies acceleration, since otherwise B could not have acquired a velocity. But it does not tell us what happens during acceleration. If we attempt to apply the FitzGerald contraction hypothesis directly to accelerated motion, we run into the Born-Herglotz theory with the consequent restriction on the number of degrees of freedom. Nevertheless the FitzGerald hypothesis has this important lesson for us. However we define a rigid body in accelerated motion, it must conform to the condition that when we pass through an accelerated state from a uniform motion to a uniform motion, the FitzGerald contraction must result.

4. Gardner's Hypothesis.

In Newtonian physics we may take hold of any particle of a rigid body and give this particle (or *drag-point* as we may call it) any motion we please. There are three degrees of freedom in the motion of the drag-point and three more in rotation about it. However (and this is important in reference to the relativistic definition given below) the drag-point has been given an artificial prominence in the above description. A motion described by means of a drag-point A may be equally well described by means of any other drag-point B selected in the body. In dynamics we usually take the mass-centre as drag-point, but that choice is dictated by dynamical considerations, not kinematical ones. From a purely kinematical standpoint, one drag-point is as good as another.

In defining a relativistic rigid body, we start with a drag-point A and assign its motion. This requires three functions, and the motion of the drag-point is represented by a world-line in space-time (the curve C in Fig. 1). This accounts for three degrees of freedom.

Consider any event P on C and let $\mathbf{L}$ be the unit tangent vector to C at P. Let $\mathbf{N}_1, \mathbf{N}_2, \mathbf{N}_3$ be a triad of unit vectors at P, orthogonal to $\mathbf{L}$ and to one another. These are 4-vectors in space-time, and orthogonality is understood in the usual sense with reference to the line-element $dx^2 + dy^2 + dz^2 - c^2dt^2$. On account of the orthogonality all the mutual scalar products of the four vectors vanish, and the squares have the following values :

$$\mathbf{L}^2 = -1, \ \mathbf{N}_1^2 = 1, \ \mathbf{N}_2^2 = 1, \ \mathbf{N}_3^2 = 1.$$

The minus sign occurs because $\mathbf{L}$ is time-like.

As P moves along C, we define a triad of $\mathbf{N}$'s at each event, arbitrary except for

the conditions stated above. There are twelve components in this triad and there are nine conditions on them, so that we are left with three free functions. These account for the other three degrees of freedom, making up the desired six. The world-line C

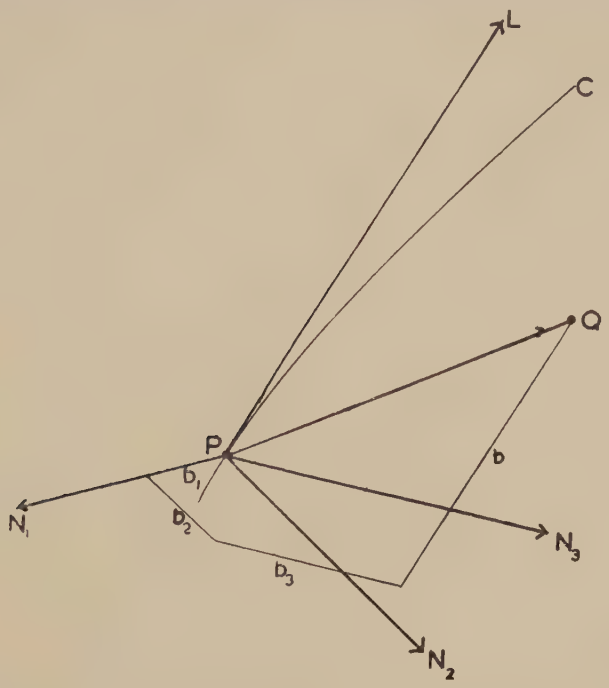

FIGURE 1

and the triad of **N**'s is the framework on which the motion of the rigid body is built.

To complete the description of the motion of the rigid body, we can proceed in at least two ways. But of these the way proposed by Mr. G. H. F. Gardner, Scholar at the Dublin Institute for Advanced Studies, seems the more interesting, and we shall confine our attention to it. To each particle of the body we attach three parameters b_1, b_2, b_3 which remain constant throughout the history of the body, zero values of the b's being attached to the drag-point. These parameters are in fact the Lagrangian co-ordinates familiar in hydrodynamics.

We now write down the formula (Gardner's hypothesis)

$$\overrightarrow{PQ} = b\mathbf{L} + b_1\mathbf{N}_1 + b_2\mathbf{N}_2 + b_3\mathbf{N}_3,$$

the first coefficient being defined as $b = (b_1^2 + b_2^2 + b_3^2)^{\frac{1}{2}}$. This formula defines the motion of the rigid body in the following way. If P is chosen on the world-line C of the drag-point and the values of the b's are assigned, this formula defines a vector $\overrightarrow{PQ}$ leading from P to an event Q (Fig. 1). If we allow P to run along C, the b's being held fixed, Q traces out a world-line. This is the history of one of the particles of the rigid body. By choosing other values for the b's we get the world-lines of the other particles of the rigid body, and so the complete motion with six degrees of freedom.

The definition, being built up in terms of the geometry of space-time, is a truly relativistic theory (i.e. invariant under Lorentz transformations).

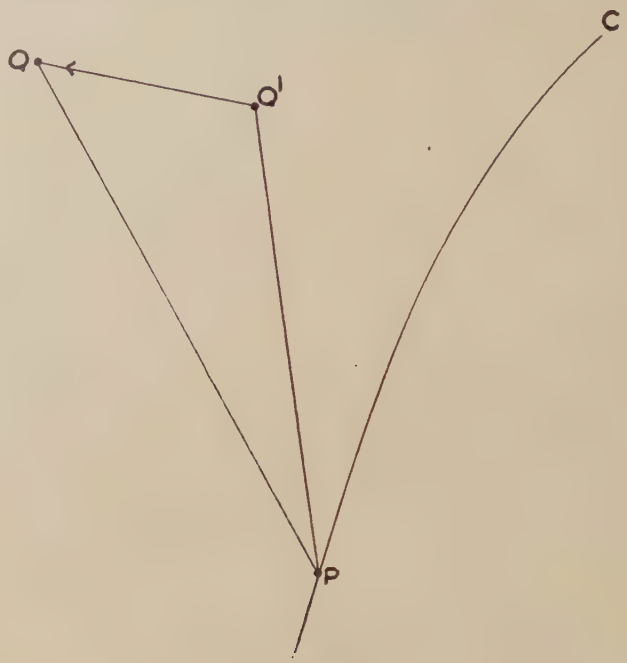

FIGURE 2

It remains to justify the use of the word " rigid " in connection with this motion. We consider (Fig. 2) two particles of the body with parameters b_1, b_2, b_3 and b_1', b_2', b_3' respectively. We have then a second formula

$$\overrightarrow{PQ'} = b'\mathbf{L} + b_1'\mathbf{N}_1 + b_2'\mathbf{N}_2 + b_3'\mathbf{N}_3,$$

and if we subtract this from the formula written above, we get for the 4-vector drawn from Q' to Q

$$\overrightarrow{Q'Q} = (b - b')\mathbf{L} + (b_1 - b_1')\mathbf{N}_1 + (b_2 - b_2')\mathbf{N}_2 + (b_3 - b_3')\mathbf{N}_3.$$

Squaring each side and making use of the orthogonalities and unit magnitudes, we get

$$Q'Q^2 = -(b - b')^2 + (b_1 - b_1')^2 + (b_2 - b_2')^2 + (b_3 - b_3')^2.$$

This is a constant for the two particles and so, as the events Q and Q' move along their respective world-lines, the space-time separation between them is preserved. This is the relativistic generalisation of the preservation of length in the Newtonian rigid body.

Another feature must be noted. If we square the formula for $\overrightarrow{PQ}$, we find that $PQ^2 = 0$. This means that $\overrightarrow{PQ}$ is a null vector, and so corresponds to the propagation of light. This means that, in a sense, rigidity is propagated with the velocity of light.

But now we must note an incompleteness in the theory. The rigid body of Gardner is unlike the Newtonian rigid body in that we cannot reproduce the same

motion using a different drag-point. The choice of one particular drag-point rather than another is no longer a matter of indifference; the motion depends on the choice, as well as on the six functions corresponding to the six degrees of freedom. Thus, although Gardner's theory is absolute in the sense that it contains no arbitrary constant to be adjusted later to fit observations, it does contain an arbitrary feature—the choice of drag-point.

5. PREDICTIONS OF THE THEORY.

The predictions of Gardner's theory with regard to fringe shifts in the Michelson-Morley experiment are worked out along the following lines.

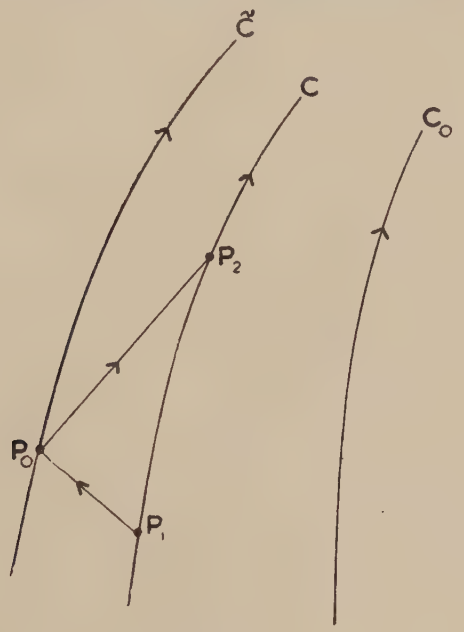

FIGURE 3

In Fig. 3, C_0 is the world-line in the drag-point, C the world-line of the source (or dividing mirror) in the interferometer and $\tilde{C}$ the world-line of one of the mirrors. (One simplifies of course by taking an interferometer with only two mirrors, one on the end of each arm). A light ray starts from the event P_1 on C, proceeds to P_0 on $\tilde{C}$, and is reflected back to P_2 on C. The trip-time T is the time elapsed between P_1 and P_2.

If we now take a second arm and denote by T_1 and T_2 the trip-times for the two arms, then the relative retardation is $T_1 - T_2$. This gives a fringe shift δ such that $\delta/w = c(T_1 - T_2)/\lambda$, where w is the fringe width and λ the wave length of the light used. The change in δ/w resulting from rotation of the interferometer is then investigated.

The calculations are straightforward but too complicated to include here. Many terms are negligibly small, and indeed all fringe shifts predicted by the theory are well below the level of observability unless the drag-point is at a considerable

distance from the interferometer. This brings us up against the arbitrary feature of the theory, namely the choice of drag-point.

Let us consider two extreme cases. Consider first an interferometer suspended as a pendulum from a support. In this case it would be natural to take the point of support as drag-point. Consider secondly an interferometer mounted on a massive vertical shaft set in a cylindrical bearing embedded in the earth. In this case we might very well regard the interferometer as forming one rigid body with the earth and having a common drag-point with the earth. But for the earth itself, regarded as a rigid body, the most natural drag-point is the earth's centre. Thus for an interferometer, integrated with the earth as described above, it appears that the earth's centre is the proper drag-point to take.

Thus, in attempting to use Gardner's theory to make a prediction about any particular interferometer, we have to assess the degree to which the instrument is insulated from, or integrated with, the earth as a whole. An analysis of Gardner's theory shows that if a sphere, originally at rest, is set in rotation, it will appear to a Galileian observer to be in a state of shearing strain, this shear being continually transmitted out from the centre with the velocity c. If the interferometer is insulated from the earth (by a pendulum suspension or something equivalent) the shear will not enter into it, and no fringe shift will be observed. On the other hand, if it is integrated with the earth, the shear will enter it and fringe shifts will occur.

For the most part experimenters have been to some pains to insulate their interferometers from the earth. Joos laid the essential part of his instrument on cross-pieces suspended by a large number of fine springs, and thereby secured a very steady fringe system. According to Gardner's theory with the above interpretation as to drag-point, Joos thereby ensured that there would be no fringe shift.

The other experimenters, including Miller, floated their instruments on mercury, and this would seem to constitute an insulation from the earth. But there was a slight difference in the case of Miller, for he used a loose pin to keep his apparatus central, whereas the others do not make any mention of a pin. Is it possible that the pin in Miller's apparatus gave it at least a partial integration with the earth ?

All we can do is to work out the predictions of Gardner's theory taking the earth's centre as drag-point and see what we get. For the ratio of fringe shift to fringe width, the theory gives (assuming the rays in the interferometer to be horizontal)

$$\delta/w = 2(D/\lambda)\,(E/c)\,\varphi \sin 2\zeta,$$

where

D = arm length in the interferometer,
λ = wave length of the light used,
E = velocity (to the east) of the interferometer, due to earth's rotation alone,
φ = inclination of the vertical to the earth's radius (deviation of the plumb line),
ζ = azimuth of telescope arm of interferometer (from north eastward).

For Miller's experiments, performed at Mount Wilson (Lat. 34° 13' N), we have in c.g.s. units

$$D/\lambda = 5.6 \times 10^7, \quad E = 3.83 \times 10^4, \quad c = 3 \times 10^{10}, \quad \varphi = 1.60 \times 10^{-3},$$

and so the prediction of Gardner's theory is

$$\delta/w = 0.23 \sin 2\zeta.$$

This gives a total shift 0.46, from a maximum at $\zeta = 45°$ (telescope arm pointing north-east) to a minimum at 135°.

These values are obtained by direct mathematical calculation from Gardner's definition of a rigid body, on the assumption that the drag-point is at the centre of the earth and that the interferometer is horizontal—that is why the deviation of the plumb line comes in. More elaborate formulae are available for the case where the rays in the interferometer are not horizontal, and reference to that case will be made later.

6. Comparison with Miller's Observations.

Miller's results have a wide dispersion, but certain average statements can be made.

Miller found a total shift 0.12 (average), as against the theoretical 0.46. He found that δ/w went through two maxima and two minima on rotating the interferometer through 360°, and a systematic harmonic analysis showed this half-period effect to be the dominant part of the variation of δ/w. Gardner's theory gives the half-period effect as the only one for a horizontal interferometer. Miller found the first maximum of δ/w to correspond to directions tending very strongly to lie in the north-easterly quadrant ; the average of the azimuths for the observations of 1 April 1925 is about 42°. Gardner's theory gives the maximum at $\zeta = 45°$ (north-east).

Miller insisted on the presence of a full-period effect, smaller than the half period effect. This is given by Gardner's theory applied to an interferometer with rays very slightly inclined to the horizontal.

Miller found certain seasonal effects. None such are given by Gardner's theory, as so far developed.

On the whole the agreement between theory and observation is rather striking. It may be noted that Miller's total shift is only about one quarter of the theoretical value ; on the theory that might be accounted for qualitatively by looseness of the pin.

As indicated above, the null results obtained by the other observers are explained by the effective insulation of their interferometers from the earth. Michelson and Morley 11 also used a pin and got shifts roughly equivalent to Miller's in magnitude and with some sign of the half-period effect.

7. Conclusion.

The question which the original Michelson-Morley experiment was designed to answer has now been satisfactorily answered according to the present view in physics.

It is impossible to detect absolute motion, and it is unlikely that anyone will ever again build an apparatus to test this impossibility.

But the question of the behaviour of physically rigid bodies in accelerated motion has not been answered to the satisfaction of everyone. It is true that the experiments referred to above do throw some light on this question, but as a rule questions are answered better by experiments designed to answer them than by experiments designed for other purposes. The Michelson-Morley experiment, as it has been performed, has been singularly poor as a check on Gardner's theory.

This is due to the horizontal position of the interferometer. If the arms were raised, the full period effect observed by Miller would, according to this theory, become very great indeed, far outweighing the half-period effect. To test Gardner's theory properly we need an interferometer firmly integrated with the earth, and with at least one of the two arms inclined to the horizontal at an angle of 45°. Such an instrument, with an arm length of only 20 cm., as compared with Miller's 3000 cm., would give a fringe shift of a full fringe width on rotation about a vertical axis. It is to be hoped that at some time such observations will be made. Whatever the result might be, it would throw light on the behaviour of a rigid body in accelerated motion.

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